SELECT WORKS

OF

THOMAS H. HUXLEY.

MAN'S PLACE IN NATURE.
THE ORIGIN OF SPECIES.
THE PHYSICAL BASIS OF LIFE.
LECTURES ON EVOLUTION.
ANIMAL AUTOMATISM.
TECHNICAL EDUCATION.

NEW YORK:
JOHN B. ALDEN, PUBLISHER.
1886.

MAN'S PLACE IN NATURE.

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EVIDENCE

AS TO

MAN'S PLACE IN NATURE.

BY

THOMAS H. HUXLEY F.R.S., F.L.S.

WITH NUMEROUS ILLUSTRATIONS.

I.

ON THE NATURAL HISTORY OF THE MAN-LIKE APES.

ANCIENT traditions, when tested by the severe processes of modern investigation, commonly enough fade away into mere dreams; but it is singular how often the dream turns out to have been a half-waking one, presaging a reality. Ovid foreshadowed the discoveries of the geologist: the Atlantis was an imagination, but Columbus found a western world; and though the quaint forms of Centaurs and Satyrs have an existence only in the realms of art, creatures approaching man more nearly than they in essential structure, and yet as thoroughly brutal as the goat's or horse's half of the mythical compound, are now not only known, but notorious.

I have not met with any notice of one of these MAN-LIKE APES of earlier date than that contained in Pigafetta's "Description of the kingdom of Congo," * drawn up from the notes of a Portuguese sailor, Eduardo Lopez, and published in 1598. The tenth chapter of this work is entitled "De Animalibus quæ in hac provincia reperiuntur," and contains a brief passage to the effect that "in the Songan country, on the banks of the Zaire, there are multitudes of apes, which afford great delight to the nobles by imitating human gestures." As this might apply to almost any kind of apes, I should have thought little of it had not the brothers De Bry, whose engravings illustrate the work, thought fit, in their eleventh "Argumentum," to figure two of these "Simiæ magnatum deliciæ." So much of the plate as contains these apes is faithfully copied in the wood-cut (Fig. 1), and it will be observed that they are tailless, long-armed, and large eared, and about the size of Chimpanzees. It may be that these apes are as much figments of the imagination of the ingenious brothers as the winged, two-legged, crocodile-headed dragon which adorns the same plate; or, on the other hand, it may

* REGNUM CONGO: hoc est VERA DESCRIPTIO REGNI AFRICANI QUOD TAM AB INCOLIS QUAM LUSITANIS, CONGUS APPELLATUR, per Philippum Pigafettam, nunc Latio sermone donata ab August. Cassiod. Reinio. Iconibus et imaginibus rerum memorabilium quasi vivis, opera et industria Joan. Theodori et Joan. Israelis de Bry fratrum exornata. Francofurti, MDXCVIII.

be that the artists have constructed their drawings from some essentially faithful description of a Gorilla or a Chimpanzee. And, in either case, though these figures are worth a passing notice, the oldest trustworthy and definite accounts of any animal of this kind date from the seventeenth century, and are due to an Englishman.

The first edition of that most amusing old book, "Purchas his Pilgrimage," was published in 1613, and therein are to be found many references to the statements of one whom Purchas terms "Andrew Battell (my neere neighbour, dwelling at Leigh in Essex) who served under Manuel Silvera Perera, Governor under the King of Spaine, at his city of Saint Paul, and with him went farre into the countrey of Angola;" and again, "my friend, Andrew Battle, who lived in the kingdom of Congo many yeares," and who, "upon some quarell betwixt the Portugals (among whom he was a sergeant of a band) and him, lived eight or nine moneths in the woods." From this weather-beaten old soldier, Purchas was amazed to hear "of a kinde of Great Apes, if they might so bee termed, of the height of a man, but twice as bigge in feature of their limmes, with strength proportionable, hairie all over, otherwise altogether like men and women in their whole bodily shape. They lived on such wilde fruits as the

FIG. 1.—Simiæ magnatum deliciæ.—De Bry, 1598.

trees and woods yielded, and in the night time lodged on the trees."

This extract is, however, less detailed and clear in its statements than a passage in the third chapter of the second part of another work—"Purchas his Pilgrimes," published in 1625, by the same author—which has been often, though hardly ever quite rightly, cited. The chapter is entitled, "The strange adventures of Andrew Battell, of Leigh in Essex, sent by the Portugals prisoner to Angola, who lived there and in the adioyning regions neere eighteene yeeres." And the sixth section of this chapter is headed, "Of the Provinces of Bongo, Calongo, Mayombe, Manikesocke, Motimbas: of the Ape Monster Pongo, their hunting: Idolatries; and divers other observations."

"This province (Calongo) toward the east bordereth upon Bongo, and toward the north upon Mayombe, which is nineteen leagues from Longo along the coast.

"This province of Mayombe is all woods and groves, so overgrowne that a man may travaile twentie days in the shadow without any sunne or heat. Here is no kind of corne nor graine, so that the people liveth onely upon plantanes and roots of sundrie sorts, very good; and nuts; nor any kinde of tame cattell nor hens.

"But they have great store of elephant's flesh, which they greatly esteeme, and many kinds of wild beasts; and great store of fish. Here is a great sandy bay, two leagues to the northward of Cape Negro, which is the port of Mayombe. Sometimes the Portugals lade logwood in this bay. Here is a great river, called Banna: in the winter it hath no barre, because the generall winds cause a great sea. But when the sunne hath his south declination, then a boat may goe in; for then it is smooth because of the raine. This river is very great, and hath many ilands and people dwelling in them. The woods are so covered with baboones, monkies, apes, and parrots, that it will feare any man to travaile in them alone. Here are also two kinds of monsters, which are common in these woods, and very dangerous.

"The greatest of these two monsters is called Pongo in their language, and the lesser is called Engeco. This Pongo is in all proportion like a man; but that he is more like a giant in stature than a man; for he is very tall, and hath a man's face, hollow-eyed, with long haire upon his browes. His face and eares are without haire, and his hands also. His bodie is full of haire, but not very thicke; and it is of a dunnish colour.

"He differeth not from a man but in his legs; for they have no calfe. Hee goeth alwaies upon his legs, and carrieth his hands clasped in the nape of his necke when he goeth upon the ground. They sleepe in the trees, and build shelters for the raine. They feed upon fruit that they find in the woods, and upon nuts, for they eate no kind of flesh. They cannot speake, and have no understanding more than a beast. The people of the countrie, when they travaile in the woods, make fires where they sleepe in the night; and in the morning when they are gone, the Pongoes will come and sit about the fire till it goeth out; for they have no understanding to lay the wood together. They goe many together, and kill many negroes that travaile in the woods. Many times they fall upon the elephants which come to feed where they be, and so beate them with their clubbed fists, and pieces of wood, that they will runne roaring away from them. Those Pongoes are never taken alive because they are so strong, that ten men cannot hold one of them; but yet they take many of their young ones with poisoned arrowes.

"The young Pongo hangeth on his mother's belly with his hands fast clasped about her, so that when the countrie people kill any of the females they take the young one, which hangeth fast upon his mother.

"When they die among themselves, they cover the dead with great heaps of boughs and wood, which is commonly found in the forest."

It does not appear difficult to identify the exact region of which Battell speaks. Longo is doubtless the name of the place usually spelled Loango on our maps. Mayombe still lies some nineteen leagues northward from Loango, along the coast; and Cilongo or Kilonga, Manikesocke, and Motimbas are yet registered by geographers. The Cape Negro of Battell, however, cannot be the modern Cape Negro in 16° S., since Loango itself is in 4° S. latitude. On the other hand, the "great river called Banna" corresponds very well with the "Camma" and "Fernand Vas," of modern geographers, which form a great delta on this part of the African coast. Now this "Camma" country is situated about a degree and a half south of the equator, while a few miles to the north of the line lies the Gaboon, and a degree or so north of that the Money River—both well known to modern naturalists as localities where the largest of man-like apes has been obtained. Moreover, at the present day, the word Engeco, or N'schego, is applied by the natives of these regions to the smaller of the two great Apes which inhabit them; so that there can be no rational doubt that Andrew Battell spoke of that which he knew of his own knowledge, or, at any rate, by immediate report from the natives of Western Africa. The "Engeco," however, is that "other monster" whose nature Battell "forgot to relate," while the name "Pongo"—applied to the animal whose characters and habits are so fully and carefully described—seems to have died out, at least in its primitive form and signification. Indeed there is evidence that not only in Battell's time, but up to a very recent date, it was used in a totally different sense from that in which he employs it.

For example, the second chapter of Purchas' work, which I have just quoted, contains "A Description and Historicall Declaration of the Golden Kingdom of Guinea, etc., etc. Translated from the Dutch, and compared also with the Latin," wherein it is stated (p. 986) that—

"The River Gaboon lyeth about fifteen miles northward from Rio de Angra, and eight miles northward from Cape de Lope Gonsalvez (Cape Lopez), and is right under the Equinoctial line, about fifteene miles from St. Thomas, and is a great land, well and easily to be knowne. At the mouth of the river there lieth a sand, three or foure fathoms deepe, whereon it beateth mightily with the streame which runneth out of the river into the sea. This river, in the mouth thereof, is at least foure miles broad; but when you are about the Iland called *Pongo* it is not above two miles broad. . . . On both sides the river there standeth many trees . . . The Iland called *Pongo*, which hath a monstrous high hill."

The French naval officers, whose letters are appended to the late M. Isidore Geoff. Saint Hilaire's excellent essay on the Gorilla,* note in similar terms the width of the Gaboon, the trees that line its banks down to the water's edge, and the strong current that sets out of it. They describe two islands in its estuary—one low, called Perroquet; the other high, presenting three conical hills, called Coniquet; and one of them, M. Franquet, expressly states that, formerly, the Chief of Coniquet was called *Meni-Pongo*, meaning thereby Lord of *Pongo*; and that the *N'Pongues* (as, in agreement with Dr. Savage, he affirms the natives call themselves) term the estuary of the Gaboon itself *N'Pongo*.

It is so easy, in dealing with savages, to misunderstand their applications of words to things, that one is at first inclined to suspect Battell of having confounded the name of this region, where his "greater monster" still abounds, with the name of the animal itself. But he is so right about other matters (including the name of the "lesser monster") that one is loath to suspect the old traveller of error; and, on the other hand, we shall find that a voyager of a hundred years' later date speaks of the name "Boggoe," as applied to a great Ape, by the inhabitants of quite another part of Africa—Sierra Leone.

But I must leave this question to be settled by philologers and travellers; and I should hardly have dwelt so long upon it except for the curious part played by this word "*Pongo*" in the later history of the man-like Apes.

The generation which succeeded Battell saw the first of the man-like Apes which was ever brought to Europe, or, at any rate,

Homo Sylvestris. Orang Outang.

FIG. 2.—The Orang of Tulpius, 1641.

whose visit found a historian. In the third

book of Tulpius' "Observationes Medicæ," published in 1641, the 56th chapter or section is devoted to what he calls *Satyrus indicus*, "called by the Indians Orang-autang, or Man-of-the-Woods, and by the Africans Quoias Morrou." He gives a very good figure, evidently from the life, of the specimen of this animal, "nostra memoria ex Angolâ delatum," presented to Frederick Henry, Prince of Orange. Tulpius says it was as big as a child of three years old, and as stout as one of six years: and that its back was covered with black hair. It is plainly a young Chimpanzee.

In the mean while the existence of other Asiatic man-like Apes became known, but at first in a very mythical fashion. Thus Bontius (1658) gives an altogether fabulous and ridiculous account and figure of an animal which he calls "Orang-outang;" and though he says, "vidi ego cujus effigiem hic exhibeo," the said effigies (see Fig. 6 for Hoppius' copy of it) is nothing but a very hairy woman of rather comely aspect, and with proportions and feet wholly human. The judicious English anatomist, Tyson, was justified in saying of this description by Bontius, "I confess I do mistrust the whole representation."

It is to the last-mentioned writer, and his coadjutor Cowper, that we owe the first account of a man-like ape which has any pretensions to a scientific accuracy and completeness. The treatise entitled, "*Orang-outang, sive Homo Sylvestris*; or the Anatomy of a Pygmie compared with that of a *Monkey*, an *Ape*, and a *Man*," published by the Royal Society in 1699, is indeed a work of remarkable merit, and has, in some respects, served as a model to subsequent inquirers. This "Pygmie," Tyson tells us, "was brought from Angola, in Africa; but was first taken a great deal higher up the country;" its hair "was of a coal-black colour, and strait," and "when it went as a quadruped on all four, 'twas awkwardly; not placing the palm of the hand flat to the ground, but it walk'd upon its knuckles, as I observed it to do when weak and had not strength enough to support its body." "From the top of the head to the heel of the foot, in a straight line, it measured twenty-six inches."

These characters, even without Tyson's good figures (Figs. 3 and 4), would have been sufficient to prove his "Pygmie" to be a young Chimpanzee. But the opportunity of examining the skeleton of the very animal Tyson anatomized having most unexpectedly presented itself to me, I am able to bear independent testimony to its being a veritable *Troglodytes niger*,* though still very young. Although fully appreciating the resemblances between his Pygmie and Man, Tyson by no means overlooked the differences between the two, and he concludes his memoir by summing up first, the points in which "the Orang-outang or Pygmie more resembled a Man than Apes and Monkeys do," under forty-seven distinct heads; and then giving, in thirty-four similar brief paragraphs, the respects in which "the Orang-outang or Pygmie differ'd from a Man and resembled more the Ape and Monkey kind."

After a careful survey of the literature of the subject extant in his time, our author arrives at the conclusion that his "Pygmie" is identical neither with the Orangs of Tulpius and Bontius, nor with the Quoias Morrou of Dapper (or rather of Tulpius), the Barris of d'Arcos, nor with the Pongo of Battell; but that it is a species of ape probably identical with the Pygmies of the Ancients, and, says Tyson, though it "does so much resemble *a Man* in many of its parts, more than any of the ape kind, or any other *animal* in the world, that I know of: yet by no means do I look upon it as the product of a *mixt* generation — 'tis a *Brute-Animal sui generis*, and a particular *species of Ape*."

The name of "Chimpanzee," by which one of the African Apes is now so well known, appears to have come into use in the first half of the eighteenth century, but the only important addition made in that period to our acquaintance with the man-like apes of Africa is contained in "A New Voyage to Guinea," by William Smith, which bears the date 1744.

In describing the animals of Sierra Leone, p. 51, this writer says:

"I shall next describe a strange sort of animal, called by the white men in this country Mandrill, but why it is so called I know not, nor did I ever hear the name before, neither can those who call them so tell, except it be for their near resemblance of a human creature, though nothing at all like an Ape. Their bodies, when full grown, are as big in circumference as a middle-sized man's—their legs much shorter, and their feet larger; their arms and hands in proportion. The head is monstrously big, and the face broad and flat, without any other hair but the eyebrows; the nose very small, the mouth wide, and the lips thin. The face, which is covered by a white skin, is monstrously ugly, being all over wrinkled as with old age; the teeth broad and yellow; the hands have no more hair than the face, but the same white skin, though all the rest of the body is covered with long black hair, like a bear. They never go upon all-fours, like apes; but cry, when vexed or teased, just like children. . . .

"When I was at Sherbro, one Mr. Cummerbus, whom I shall have occasion hereafter to mention, made me a present of one of these strange animals, which are called by the natives Boggoe: it was a she-cub, of six months' age, but even then larger than a Baboon. I gave it in charge to one of the slaves, who knew how to feed and nurse it, being a very tender sort of animal; but whenever I went off the deck the sailors began to teaze it—some loved to see its tears and hear it cry; others hated its snotty-nose; one who hurt it, being checked by the negro that took care of it, told the slave he was very fond of his country-woman, and asked

FIGS. 3 and 4.—The "Pygmie" reduced from Tyson's figures 1 and 2, 1699.

FIG. 5.—Facsimile of William Smith's figure of the "Mandrill," 1744.

him if he should not like her for a wife? To which the slave very readily replied, 'No, this no my wife; this a white woman—this fit wife for you.' This unlucky wit of the negro's, I fancy, hastened its death, for next morning it was found dead under the windlass."

William Smith's "Mandrill," or "Boggoe," as his description and figure testify, was, without doubt, a Chimpanzee.

Linnæus knew nothing, of his own observation, of the man-like Apes of either Africa or Asia, but a dissertation by his pupil Hoppius in the "Amœnitates Academicæ" (VI. "Anthropomorpha") may be regarded as embodying his views respecting these animals.

The dissertation is illustrated by a plate, of which the accompanying wood-cut, Fig. 6, is a reduced copy. The figures are entitled (from left to right), 1. *Troglodyta Bontii*; 2. *Lucifer Aldrovandi*; 3. *Satyrus Tulpii*; 4. *Pygmæus Edwardi*. The first is a bad copy of Bontius' fictitious "Orang-

outang," in whose existence, however, Linnæus appears to have fully believed; for in the standard edition of the "Systema Naturæ" it is enumerated as a second species of Homo; "H. nocturnus." *Lucifer Aldrovandi* is a copy of a figure in Aldrovandus, "De Quadrupedibus digitatis viviparis," Lib. 2, p. 249 (1645), entitled, "Cercopithecus formæ raræ *Barbilius* vocatus et originem a China ducebat." Hoppius is of opinion that this may be one of that cat-tailed people of whom Nicolaus Köping affirms that they eat a boat's crew, "gubernator navis" and all! In the "Systema naturæ," Linnæus calls it, in a note, *Homo caudatus*, and seems inclined to regard it as a third species of man. According to Temminck, *Satyrus Tulpii* is a copy of the figure of a Chimpanzee published by Scotin in 1738, which I have not seen. It is the *Satyrus indicus* of the "Systema Naturæ," and is regarded by Linnæus as possibly a distinct species from *Satyrus sylvestris*. The last, named *Pygmæus Edwardi*, is copied from the figure of a young "Man of the Woods," or true Orang-Utan, given in Edwards' "Gleanings of Natural History" (1758).

Buffon was more fortunate than his great rival. Not only had he the rare opportunity of examining a young Chimpanzee in the living state, but he became possessed of an adult Asiatic man-like Ape—the first and the last adult specimen of any of these animals brought to Europe for many years. With the valuable assistance of Daubenton, Buffon gave an excellent description of this creature, which, from its singular proportions, he termed the long-armed Ape, or Gibbon. It is the modern *Hylobates lar*.

Thus when, in 1766, Buffon wrote the fourteenth volume of his great work, he was personally familiar with the young of one kind of African man-like Ape, and with the adult of an Asiatic species—while the Orang-Utan and the Mandrill of Smith were known to him by report. Furthermore, the Abbé Prevost had translated a good deal of Purchas' Pilgrims into French, in his "Histoire générale des Voyages" (1748), and there Buffon found a version of Andrew Battell's account of the Pongo and the Engeco. All these data Buffon attempts to weld together into harmony in his chapter entitled "Les Orang-outangs ou le Pongo et le Jocko." To this title the following note is appended:

"Orang-outang, nom de cet animal aux Indes orientales: Pongo, nom de cet animal à Lowando Province de Congo.

"Jocko, Enjocko, nom de cet animal à Congo que nous avons adopté. *En* est l article que nous avons retranché."

Thus it was that Andrew Battell's "Engeco" became metamorphosed into "Jocko," and, in the latter shape, was spread all over the world, in consequence of the extensive popularity of Buffon's works. The Abbé Prevost and Buffon between them, however, did a good deal more disfigurement to Battell's sober account than "cutting off an article." Thus Battell's statement that the Pongos "cannot speake, and have no understanding more than a beast," is rendered by Buffon "qu'il ne peut parler *quoiqu'il ait plus d'entendement que les autres animaux*;" and again, Purchas' affirmation, "He told me in conference with him that one of these Pongos tooke a negro boy of his which lived a moneth with them," stands in the French version, "un pongo lui enleva un petit nègre qui passa un *an* entier dans la société de ces animaux."

After quoting the account of the great Pongo, Buffon justly remarks that all the "Jockos" and "Orangs" hitherto brought to Europe were young; and he suggests that, in their adult condition, they might be as big as the Pongo or "great Orang;" so that, provisionally, he regarded the Jockos, Orangs, and Pongos, as all of one species. And perhaps this was as much as the state of knowledge at the time warranted. But how it came about that Buffon failed to perceive the similarity of Smith's "Mandrill" to his own "Jocko," and confounded the former with so totally different a creature as the blue-faced Baboon, is not so easily intelligi-

FIG. 6.—The Anthropomorpha of Linnæus.

ble.

Twenty years later Buffon changed his opinion, and expressed his belief that the Orangs constituted a genus with two species —a large one, the Pongo of Battell, and a small one, the Jocko: that the small one (Jocko) is the East Indian Orang; and that the young animals from Africa, observed by himself and Tulpius, are simply young Pongos.

In the mean while the Dutch naturalist, Vosmaer gave, in 1778, a very good account and figure of a young Orang brought alive to Holland, and his countryman, the famous anatomist, Peter Camper, published (1779) an essay on the Orang-Utan of similar value to that of Tyson on the Chimpanzee. He dissected several females and a male, all of which, from the state of their skeleton and their dentition, he justly supposes to have been young. However, judging by the analogy of man, he concludes that they could not have exceeded four feet in height in the adult condition. Furthermore, he is very clear as to the specific distinctness of the true East Indian Orang.

"The Orang," says he, "differs not only from the Pigmy of Tyson and from the Orang of Tulpius by its peculiar color and its long toes, but also by its whole external form. Its arms, its hands, and its feet are longer, while the thumbs, on the contrary, are much shorter, and the great toes much smaller in proportion." And again, "The true Orang, that is to say, that of Asia, that of Borneo, is consequently not the Pithecus, or tailless Ape, which the Greeks, and especially Galen, have described. It is neither the Pongo nor the Jocko, nor the Orang of Tulpius, nor the Pigmy of Tyson—*it is an animal of a peculiar species*, as I shall prove in the clearest manner by the organs of voice and the skeleton, in the following chapters"

A few years later, M. Radermacher, who held a high office in the Government of the Dutch dominions in India, and was an active member of the Batavian Society of Arts and Sciences, published, in the second part of the Transactions of that Society, a Description of the Island of Borneo, which was written between the years 1779 and 1781, and, among much other interesting matter, contains some notes upon the Orang. The small sort of Orang-Utan, viz., that of Vosmaer and of Edwards, he says, is found only in Borneo, and chiefly about Banjermassing, Mampauwa, and Landak. Of these he had seen some fifty during his residence in the Indies; but none exceeded 2½ feet in length. The larger sort, often regarded as chimera, continues Radermacher, would perhaps long have remained so had it not been for the exertions of the Resident at Rembang, M. Palm, who, on returning from Landak toward Pontiana, shot one, and forwarded it to Batavia in spirit, for transmission to Europe.

Palm's letter describing the capture runs thus: "Herewith I send your Excellency, contrary to all expectation (since long ago I offered more than a hundred ducats to the natives for an Orang-Utan of four or five feet high) an Orang which I heard of this morning about eight o'clock. For a long time we did our best to take the frightful beast alive in the dense forest about half way to Landak. We forgot even to eat, so anxious were we not to let him escape; but it was necessary to take care he did not revenge himself, as he kept continually breaking off heavy pieces of wood and green branches, and dashing them at us. This game lasted till four o'clock in the afternoon, when we determined to shoot him; in which I succeeded very well, and indeed better than I ever shot from a boat before; for the bullet went just into the side of his chest, so that he was not much damged. We got him into the prow still living, and bound him fast, and next morning he died of his wounds. All Pontiana came on board to see him when we arrived." Palm gives his height from the head to the heel as 49 inches.

A very intelligent German officer, Baron Von Wurmb, who at this time held a post in the Dutch East India service, and was Secretary of the Batavian Society, studied this animal, and his careful description of it, entitled "Beschrijving van der Groote Borneosche Orang-outang of de Oost-Indische Pongo," is contained in the same volume of the Batavian Society's Transactions. After Von Wurmb had drawn up his description he states, in a letter dated Batavia, February 18th, 1781, that the specimen was sent to Europe in brandy to be placed in the collection of the Prince of Orange; "unfortunately," he continues, "we hear that the ship has been wrecked." Von Wurmb died in the course of the year 1781, the letter in which this passage occurs being the last he wrote; but in his posthumous papers, published in the fourth part of the Transactions of the Batavian Society, there is a brief description, with measurements, of a female Pongo four feet high.

Did either of these original specimens, on which Von Wurmb's descriptions are based, ever reach Europe? It is commonly supposed that they did; but I doubt the fact. For, appended to the memoir "De l'Ourang-outang," in the collected edition of Camper's works, Tome I., pp. 64-66, is a note by Camper himself, referring to Von Wurmb's papers, and continuing thus: "Heretofore, this kind of ape had never been known in Europe. Radermacher has had the kindness to send me the skull of one of these animals, which measured fifty-three inches, or four feet five inches in height. I have sent some sketches of it to M. Soemmering at Mayence, which are better calculated, however, to give an idea of the form than of the real size of the parts."

These sketches have been reproduced by Fischer and by Lucæ, and bear date 1783, Soemmering having received them in 1784. Had either of Von Wurmb's specimens reached Holland, they would hardly have

been unknown at this time to Camper, who, however, goes on to say: "It appears that since this some more of these monsters have been captured, for an entire skeleton, very badly set up, which had been sent to the Museum of the Prince of Orange, and which I saw only on the 27th of June, 1784, was more than four feet high. I examined this skeleton again on the 19th December, 1785, after it had been excellently put to rights by the ingenious Onymos."

It appears evident, then, that this skeleton, which is doubtless that which has always gone by the name of Wurmb's Pongo, is not that of the animal described by him, though unquestionably similar in all essential points.

Camper proceeds to note some of the most important features of this skeleton; promises to describe it in detail by and by; and is evidently in doubt as to the relation of this great "Pongo" to his "petit Orang."

The promised further investigations were never carried out; and so it happened that the Pongo of Von Wurmb took its place by the side of the Chimpanzee, Gibbon, and Orang as a fourth and colossal species of man-like Ape. And indeed nothing could look much less like the Chimpanzees or the Orangs, then known, than the Pongo; for all the specimens of Chimpanzee and Orang which had been observed were small of stature, singularly human in aspect, gentle, and docile; while Wurmb's Pongo was a monster almost twice their size, of vast strength and fierceness, and very brutal in expression, its great projecting muzzle, armed with strong teeth, being further disfigured by the outgrowth of the cheeks into fleshy lobes.

Eventually, in accordance with the usual marauding habits of the Revolutionary armies, the "Pongo" skeleton was carried away from Holland into France, and notices of it, expressly intended to demonstrate its entire distinctness from the Orang, and its affinity with the baboons, were given, in 1798, by Geoffroy St. Hilaire and Cuvier.

Even in Cuvier's "Tableau Elementaire," and in the first edition of his great work, the "Règne Animal," the "Pongo" is classed as a species of Baboon. However, so early as 1818 it appears that Cuvier saw reason to alter this opinion, and to adopt the view suggested several years before by Blumenbach, and after him by Tilesius, that the Bornean Pongo is simply an adult Orang. In 1824 Rudolphi demonstrated by the condition of the dentition more fully and completely than had been done by his predecessors that the Orangs described up to that time were all young animals, and that the skull and teeth of the adult would probably be such as those seen in the Pongo of Wurmb. In the second edition of the "Règne Animal" (1829) Cuvier infers from the "proportions of all the parts" and "the arrangements of the foramina and sutures of the head" that the Pongo is the adult of the Orang-Utan, "at least of a very closely allied species," and this conclusion was eventually placed beyond all doubt by Professor Owen's Memoir, published in the "Zoological Transactions" for 1835, and by Temminck in his "Monographies de Mammalogie." Temminck's memoir is remarkable for the completeness of the evidence which it affords as to the modification which the form of the Orang undergoes according to age and sex. Tiedemann first published an account of the brain of the young Orang, while Sandifort Müller and Schlegel described the muscles and the viscera of the adult, and gave the earliest detailed and trustworthy history of the habits of the great Indian Ape in a state of nature; and as important additions have been made by later observers, we are at this moment better acquainted with the adult of the Orang-Utan than with that of any of the other greater man-like Apes.

It is certainly the Pongo of Wurmb; and it is as certainly not the Pongo of Battell, seeing that the Orang-Utan is entirely confined to the great Asiatic islands of Borneo and Sumatra.

And while the progress of discovery thus cleared up the history of the Orang, it also

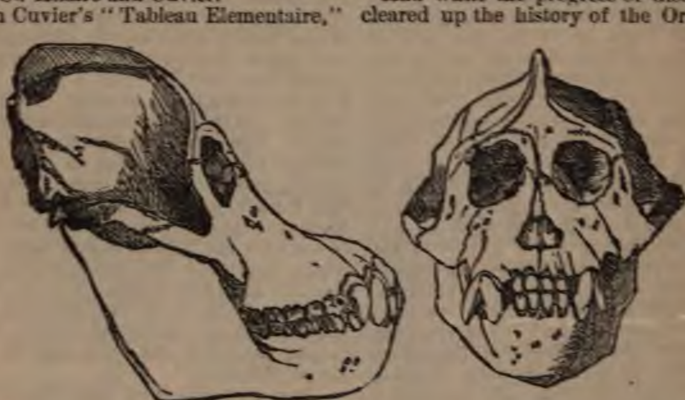


FIG. 7.—The Pongo Skull, sent by Radermacher to Camper, after Camper's original sketches, as reproduced by Lucæ.

became established that the only other man-like Apes in the Eastern world were the various species of Gibbon—Apes of smaller stature, and therefore attracting less attention than the Orangs, though they are spread over a much wider range of country, and are hence more accessible to observation.

Although the geographical area inhabited by the "Pongo" and "Engeco" of Battell is so much nearer to Europe than that in which the Orang and Gibbon are found, our acquaintance with the African Apes has been of slower growth; indeed it is only within the last few years that the truthful story of the old English adventurer has been rendered fully intelligible. It was not until 1835 that the skeleton of the adult Chimpanzee became known, by the publication of Professor Owen's above-mentioned very excellent memoir, "On the Osteology of the Chimpanzee and Orang," in the Zoological Transactions—a memoir which, by the accuracy of its descriptions, the carefulness of its comparisons, and the excellence of its figures, made an epoch in the history of our knowledge of the bony framework, not only of the Chimpanzee, but of all the anthropoid Apes.

By the investigations herein detailed, it became evident that the old Chimpanzee acquired a size and aspect as different from those of the young known to Tyson, to Buffon, and to Traill, as those of the old Orang from the young Orang; and the subsequent very important researches of Messrs. Savage and Wyman, the American missionary and anatomist, have not only confirmed this conclusion, but have added many new details.

One of the most interesting among the many valuable discoveries made by Dr. Thomas Savage is the fact that the natives in the Gaboon country at the present day apply to the Chimpanzee a name—"Enché-eko"—which is obviously identical with the "Engeko" of Battell, a discovery which has been confirmed by all later inquirers. Battell's "lesser monster" being thus proved to be a veritable existence, of course a strong presumption arose that his "greater monster," the "Pongo," would sooner or later be discovered. And indeed a modern traveller, Bowdich, had, in 1819, found strong evidence among the natives of the existence of a second great Ape, called the "Ingena," "five feet high, and four across the shoulders," the builder of a rude house, on the outside of which it slept.

In 1847 Dr. Savage had the good fortune to make another and most important addition to our knowlege of the man-like Apes; for, being unexpectedly detained at the Gaboon River, he saw in the house of the Rev. Mr. Wilson, a missionary resident there, "a skull represented by the natives to be a monkey-like animal, remarkable for its size, ferocity, and habits." From the contour of the skull, and the information derived from several intelligent natives, "I was induced," says Dr. Savage (using the term Orang in its old general sense), "to believe that it belonged to a new species of Orang. I expressed this opinion to Mr. Wilson, with a desire for further investigation; and, if possible, to decide the point by the inspection of a specimen, alive or dead." The result of the combined exertions of Messrs. Savage and Wilson was not only the obtaining of a very full account of the habits of this new creature, but a still more important service to science, the enabling the excellent American anatomist already mentioned, Professor Wyman, to describe, from ample materials, the distinctive osteological characters of the new form. This animal was called by the natives of the Gaboon "Engé-ena," a name obviously identical with the "Ingena" of Bowdich; and Dr. Savage arrived at the conviction that this last discovered of all the great Apes was the long-sought "Pongo" of Battell.

The justice of this conclusion indeed is beyond doubt—for not only does the "Engé-ena" agree with Battell's "greater monster" in its hollow eyes, its great stature, and its dun or iron-gray color, but the only other man-like Ape which inhabits these latitudes—the Chimpanzee—is at once identified, by its smaller size, as the "lesser monster," and is excluded from any possibility of being the "Pongo," by the fact that it is black and not dun, to say nothing of the important circumstance already mentioned that it still retains the name of "Engeko" or "Enché-eko," by which Battell knew it.

In seeking for a specific name for the "Enge-ena," however, Dr. Savage wisely avoided the much misused "Pongo:" but finding in the ancient Periplus of Hanno the word "Gorilla," applied to certain hairy savage people, discovered by the Carthaginian voyager in an island on the African coast, he attached the specific name "*Gorilla*" to his new ape, whence arises its present well-known appellation. But Dr. Savage, more cautious than some of his successors, by no means identifies his ape with Hanno's "wild men." He merely says that the latter were "probably one of the species of the Orang;" and I quite agree with M. Brullé that there is no ground for identifying the modern "Gorilla" with that of the Carthaginian admiral.

Since the memoir of Savage and Wyman was published, the skeleton of the Gorilla has been investigated by Professor Owen and by the late Professor Duvernoy, of the Jardin des Plantes, the latter having further supplied a valuable account of the muscular system and of many of the other soft parts; while African missionaries and travellers have confirmed and expanded the account originally given of the habits of this great man-like Ape, which has had the singular fortune of being the first to be made known to the general world and the last to be scientifically investigated.

Two centuries and a half have passed away since Battell told his stories about the "greater" and the "lesser monsters" to Purchas, and it has taken nearly that time

Photographically reduced from diagrams of the natural size (except that of the Gibbon, which was twice as large as nature), drawn by Mr. Waterhouse Hawkins from specimens in the Museum of the Royal College of Surgeons.

GIBBON. ORANG. CHIMPANZEE. GORILLA. MAN.

FIG. 8.—Skeletons of

to arrive at the clear result that there are four distinct kinds of Anthropoids—in Eastern Asia, the Gibbons and the Orangs; in Western Africa, the Chimpanzees and the Gorilla.

The man-like Apes, the history of whose discovery has just been detailed, have certain characters of structure and of distribution in common. Thus, they all have the same number of teeth as man—possessing four incisors, two canines, four false molars, and six true molars in each jaw, or 32 teeth in all, in the adult condition; while the milk dentition consists of 20 teeth—or four incisors, two canines, and four molars in each jaw. They are what are called catarrhine Apes—that is, their nostrils have a narrow partition and look downward; and, furthermore, their arms are always longer than their legs, the difference being sometimes greater and sometimes less; so that if the four were arranged in the order of the length of their arms in proportion to that of their legs, we should have this series—Orang ($1\frac{2}{3}$—1), Gibbon ($1\frac{1}{4}$—1), Gorilla ($1\frac{1}{5}$—1), Chimpanzee ($1\frac{1}{16}$—1). In all, the fore limbs are terminated by hands, provided with longer or shorter thumbs; while the great toe of the foot, always smaller than in Man, is far more movable than in him, and can be opposed, like a thumb, to the rest of the foot. None of these apes have tails, and none of them possess the cheek-pouches common among monkeys. Finally, they are all inhabitants of the old world.

The Gibbons are the smallest, slenderest, and longest-limbed of the man-like Apes; their arms are longer in proportion to their bodies than those of any of the other man-like Apes, so that they can touch the ground when erect; their hands are longer than their feet, and they are the only Anthropoids which possess callosities like the lower monkeys. They are variously colored. The Orangs have arms which reach to the ankles in the erect position of the animal; their thumbs and great toes are very short, and their feet are longer than their hands. They are covered with reddish-brown hair, and the sides of the face, in adult males, are commonly produced into two crescentic, flexible excrescences, like fatty tumors. The Chimpanzees have arms which reach below the knees; they have large thumbs and great toes, their hands are longer than their feet, and their hair is black, while the skin of the face is pale. The Gorilla, lastly, has arms which reach to the middle of the leg, large thumbs, and great toes, feet longer than the hands, a black face, and dark-gray or dun hair.

For the purpose which I have at present in view, it is unnecessary that I should enter into any further minutiæ respecting the distinctive characters of the genera and species into which these man-like Apes are divided by naturalists. Suffice it to say, that the Orangs and the Gibbons constitute the distinct genera, *Simia* and *Hylobates*; while the Chimpanzees and Gorillas are by some regarded simply as distinct species of one genus, *Troglodytes*; by others as distinct genera—*Troglodytes* being reserved for the Chimpanzees, and *Gorilla* for the Engé-ena or Pongo.

Sound knowledge respecting the habits and mode of life of the man-like Apes has been even more difficult of attainment than correct information regarding their structure.

Once in a generation, a Wallace may be found physically, mentally, and morally qualified to wander unscathed through the tropical wilds of America and of Asia, to form magnificent collections as he wanders, and withal to think out sagaciously the conclusions suggested by his collections; but, to the ordinary explorer or collector, the dense forests of equatorial Asia and Africa, which constitute the favorite habitation of the Orang, the Chimpanzee, and the Gorilla, present difficulties of no ordinary magnitude; and the man who risks his life by even a short visit to the malarious shores of those regions may well be excused if he shrinks from facing the dangers of the interior; if he contents himself with stimulating the industry of the better-seasoned natives, and collecting and collating the more or less mythical reports and traditions with which they are too ready to supply him.

In such a manner most of the earlier accounts of the habits of the man-like Apes originated; and even now a good deal of what passes current must be admitted to have no very safe foundation. The best information we possess is that based almost wholly on direct European testimony respecting the Gibbons; the next best evidence relates to the Orangs; while our knowledge of the habits of the Chimpanzee and the Gorilla stands much in need of support and enlargement by additional testimony from instructed European eye-witnesses.

It will therefore be convenient in endeavoring to form a notion of what we are justified in believing about these animals, to commence with the best known man-like Apes, the Gibbons, and Orangs; and to make use of the perfectly reliable information respecting them as a sort of criterion of the probable truth or falsehood of assertions respecting the others.

Of the GIBBONS, half a dozen species are found scattered over the Asiatic islands, Java, Sumatra, Borneo, and through Malacca, Siam, Arracan, and an uncertain extent of Hindostan on the main-land of Asia. The largest attain a few inches above three feet in height, from the crown to the heel, so that they are shorter than the other man-like Apes, while the slenderness of their bodies renders their mass far smaller in proportion even to this diminished height.

Dr. Salomon Müller, an accomplished Dutch naturalist, who lived for many years in the Eastern Archipelago, and to the result of whose personal experience I shall frequently have occasion to refer, states that the Gibbons are true mountaineers, loving

the slopes and edges of the hills, though they rarely ascend beyond the limit of the fig-trees. All day long they haunt the tops of the tall trees, and though, toward evening, they descend in small troops to the open ground, no sooner do they spy a man than they dart up the hillsides and disappear in the darker valleys.

FIG. 9.—A Gibbon (*H. pileatus*), after Wolf

All observers testify to the prodigious volume of voice possessed by these animals. According to the writer whom I have just cited, in one of them, the Siamang, "the voice is grave and penetrating, resembling the sounds gōek, gōek, gōek, gōek, goek ha ha ha ha haāāāā, and may be easily heard at a distance of half a league." While the cry is being uttered, the great membranous bag under the throat which communicates with the organ of voice, the so-called "laryngeal sac," becomes greatly distended, diminishing again when the creature relapses into silence.

M. Duvaucel, likewise, affirms that the cry of the Siamang may be heard for miles—making the woods ring again. So Mr. Martin describes the cry of the agile Gibbon as "overpowering and deafening" in a room, and "from its strength, well calculated for resounding through the vast forests." Mr. Waterhouse, an accomplished musician as well as zoologist, says, "The Gibbon's voice is certainly much more powerful than that of any singer I ever heard." And yet it is to be recollected that this animal is not half the height of, and far less bulky in proportion than, a man.

There is good testimony that various species of Gibbon readily take to the erect posture. Mr. George Bennett, a very excellent observer, in describing the habits of a male *Hylobates syndactylus* which remained for some time in his possession, says: "He invariably walks in the erect posture when on a level surface; and then the arms either hang down, enabling him to assist himself with his knuckles; or, what is more usual, he keeps his arms uplifted in nearly an erect position, with the hands pendent ready to seize a rope, and climb up on the approach of danger or on the obtrusion of strangers. He walks rather quick in the erect posture, but with a waddling gait, and is soon run down if, while pursued, he has no opportunity of escaping by climbing. . . . When he walks in the erect posture, he turns the leg and foot outward, which occasions him to have a waddling gait and to seem bow-legged."

Dr. Burrough states of another Gibbon, the Horlack or Hooluk:

"They walk erect; and when placed on the floor, or in an open field, balance themselves very prettily by raising their hands over their head and slightly bending the arm at the wrist and elbow, and then run tolerably fast, rocking from side to side; and, if urged to greater speed, they let fall their hands to the ground, and assist themselves forward, rather jumping than running, still keeping the body, however, nearly erect."

Somewhat different evidence, however, is given by Dr. Winslow Lewis:

"Their only manner of walking was on their posterior or inferior extremities, the others being raised upward to preserve their equilibrium, as rope-dancers are assisted by long poles at fairs. Their progression was not by placing one foot before the other, but by simultaneously using both, as in jumping." Dr. Salomon Müller also states that the Gibbons progress upon the ground by short series of tottering jumps, effected only by the hind limbs, the body being held altogether upright.

But Mr. Martin (l. c. p. 418), who also speaks from direct observation, says of the Gibbons generally:

"Pre-eminently qualified for arboreal habits, and displaying among the branches amazing activity, the Gibbons are not so awkward or embarrassed on a level surface as might be imagined. They walk erect,

with a waddling or unsteady gait, but at a quick pace, the equilibrium of the body requiring to be kept up, either by touching the ground with the knuckles, first on one side then on the other, or by uplifting the arms so as to poise it. As with the Chimpanzee, the whole of the narrow, long sole of the foot is placed upon the ground at once, and raised at once, without any elasticity of step."

After this mass of concurrent and independent testimony, it cannot reasonably be doubted that the Gibbons commonly and habitually assume the erect attitude.

But level ground is not the place where these animals can display their very remarkable and peculiar locomotive powers, and that prodigious activity which almost tempts one to rank them among flying, rather than among ordinary climbing mammals.

Mr. Martin (l. c. p. 430) has given so excellent and graphic an account of the movements of a *Hylobates agilis*, living in the Zoological Gardens, in 1840, that I will quote it in full:

"It is almost impossible to convey in words an idea of the quickness and graceful address of her movements: they may indeed be termed aerial, as she seems merely to touch in her progress the branches among which she exhibits her evolutions. In these feats her hands and arms are the sole organs of locomotion, her body hanging as if suspended by a rope, sustained by one hand (the right, for example), she launches herself, by an energetic movement, to a distant branch, which she catches with the left hand; but her hold is less than momentary; the impulse for the next launch is acquired; the branch then aimed at is attained by the right hand again, and quitted instantaneously, and so on, in alternate succession. In this manner spaces of twelve and eighteen feet are cleared, with the greatest ease and uninterruptedly, for hours together, without the slightest appearance of fatigue being manifested; and it is evident that, if more space could be allowed, distances very greatly exceeding eighteen feet would be as easily cleared; so that Duvaucel's assertion that he has seen these animals launch themselves from one branch to another, forty feet asunder, startling as it is, may be well credited. Sometimes, on seizing a branch in her progress, she will throw herself, by the power of one arm only, completely round it, making a revolution with such rapidity as almost to deceive the eye, and continue her progress with undiminished velocity. It is singular to observe how suddenly this Gibbon can stop, when the impetus given by the rapidity and distance of her swinging leaps would seem to require a gradual abatement of her movements. In the very midst of her flight a branch is seized, the body raised, and she is seen, as if by magic, quietly seated on it, grasping it with her feet. As suddenly she again throws herself into action.

"The following facts will convey some notion of her dexterity and quickness. A live bird was let loose in her apartment: she marked its flight, made a long swing to a distant branch, caught the bird with one hand in her passage, and attained the branch with her other hand, her aim, both at the bird and at the branch, being as successful as if one object only had engaged her attention. It may be added that she instantly bit off the head of the bird, picked its feathers, and then threw it down without attempting to eat it.

"On another occasion this animal swung herself from a perch, across a passage at least twelve feet wide, against a window which it was thought would be immediately broken: but not so; to the surprise of all, she caught the narrow framework between the panes with her hand, in an instant attained the proper impetus, and sprang back again to the cage she had left—a feat requiring not only great strength, but the nicest precision."

The Gibbons appear to be naturally very gentle, but there is very good evidence that they will bite severely when irritated, a female *Hylobates agilis* having so severely lacerated one man with her long canines that he died; while she had injured others so much that, by way of precaution, these formidable teeth had been filed down; but if threatened she would still turn on her keeper. The Gibbons eat insects, but appear generally to avoid animal food. A Siamang, however, was seen by Mr. Bennett to seize and devour greedily a live lizard. They commonly drink by dipping their fingers in the liquid and then licking them. It is asserted that they sleep in a sitting posture.

Duvaucel affirms that he has seen the females carry their young to the water-side and there wash their faces, in spite of resistance and cries. They are gentle and affectionate in captivity—full of tricks and pettishness, like spoiled children, and yet not devoid of a certain conscience, as an anecdote, told by Mr. Bennett (l. c. p. 156) will show. It would appear that his Gibbon had a peculiar inclination for disarranging things in the cabin. Among these articles a piece of soap would especially attract his notice, and for the removal of this he had been once or twice scolded. "One morning," says Mr. Bennett, "I was writing, the Ape being present in the cabin, when, casting my eyes toward him, I saw the little fellow taking the soap. I watched him without his perceiving that I did so: and he occasionally would cast a furtive glance toward the place where I sat. I pretended to write; he, seeing me busily occupied, took the soap, and moved away with it in his paw. When he had walked half the length of the cabin, I spoke quietly, without frightening him. The instant he found I saw him he walked back again and deposited the soap nearly in the same place from whence he had taken it. There was certainly something more than instinct in that action: he evidently betrayed a consciousness of having done wrong both by his first and last actions—and what is reason if that

is not an exercise of it?"

The most elaborate account of the natural history of the Orang-Utan extant is that given in the "Verhandelingen over de Natuurlijke Geschiedenis der Nederlandsche overzeesche Bezittingen (1839–45)," by Dr. Salomon Müller and Dr. Schlegel, and I shall base what I have to say upon this subject almost entirely on their statements, adding here and there particulars of interest from the writings of Brooke, Wallace, and others.

The Orang-Utan would rarely seem to exceed four feet in height, but the body is very bulky, measuring two thirds of the height in circumference.

The Orang-Utan is found only in Sumatra and Borneo, and is common in neither of these islands—in both of which it occurs always in low, flat plains, never in the mountains. It loves the densest and most sombre of the forests, which extend from the sea-shore inland, and thus is found only in the eastern half of Sumatra, where alone such forests occur, though, occasionally, it strays over to the western side.

On the other hand it is generally distributed through Borneo, except in the mountains, or where the population is dense. In favorable places the hunter may, by good fortune, see three or four in a day.

Except in the pairing time, the old males usually live by themselves. The old females and the immature males, on the other hand, are often met with in twos and threes; and the former occasionally have young with them, though the pregnant females usually separate themselves, and sometimes remain apart after they have given birth to their offspring. The young Orangs seem to remain unusually long under their mother's protection, probably in consequence of their slow growth. While climbing the mother always carries her young against her bosom, the young holding on by his mother's hair. At what time of life the Orang-Utan becomes capable of propagation, and how long the females go with young is unknown, but it is probable that they are not adult until they arrive at ten or fifteen years of age. A female which lived for five years at Batavia had not attained one third the height of the wild females. It is probable that, after reaching adult years, they go on growing, though slowly, and that they live to forty or fifty years. The Dyaks tell of old Orangs which have not only lost all their teeth, but which find it so troublesome to climb that they maintain themselves on windfalls and juicy herbage.

The Orang is sluggish, exhibiting none of that marvellous activity characteristic of the Gibbons. Hunger alone seems to stir him to exertion, and when it is stilled he relapses into repose. When the animal sits, it curves its back and bows its head so as to look straight down on the ground; sometimes it holds on with its hands by a higher branch, sometimes lets them hang phlegmatically down by its side; and in these positions the Orang will remain for hours together, in the same spot, almost without stirring, and only now and then giving utterance to its deep, growling voice. By day he usually climbs from one tree-top to another, and only at night descends to the ground; and if then threatened with danger he seeks refuge among the underwood. When not hunted, he remains a long time in the same locality, and sometimes stops for many days on the same tree, a firm place among its branches serving him for a bed. It is rare for the Orang to pass the night in the summit of a large tree, probably because it is too windy and cold there for him; but as soon as night draws on he descends from the height and seeks out a fit bed in the lower and darker part, or in the leafy top of a small tree, among which he prefers Nibong palms, Pandani, or one of those parasitic orchids which give the primeval forests of Borneo so characteristic and striking an appearance. But wherever he determines to sleep, there he prepares himself a sort of nest; little boughs and leaves are drawn together round the selected spot, and bent crosswise over one another; while to make the bed soft, great leaves of ferns, of orchids, of *Pandanus fascicularis*, *Nipa fruticans*, *etc.*, are laid over them. Those which Müller saw, many of them being very fresh, were situated at a height of ten to twenty-five feet above the ground, and had a circumference, on the average, of two or three feet. Some were packed many inches thick with *pandanus* leaves; others were remarkable only for the cracked twigs, which, united in a common centre, formed a regular platform. "The rude *hut*," says Sir James Brooke, "which they are stated to build in the trees, would be more properly called a seat or nest, for it has no roof or cover of any sort. The facility with which they form this nest is curious, and I had an opportunity of seeing a wounded female weave the branches together and seat herself within a minute."

According to the Dyaks the Orang rarely leaves his bed before the sun is well above the horizon and has dissipated the mists. He gets up about nine, and goes to bed again about five; but sometimes not till late in the twilight. He lies sometimes on his back, or, by way of change, turns on one side or the other, drawing his limbs up to his body, and resting his head on his hand. When the night is cold, windy, or rainy, he usually covers his body with a heap of *pandanus*, *nipa*, or fern leaves, like those of which his bed is made, and he is especially careful to wrap up his head in them. It is this habit of covering himself up which has probably led to the fable that the Orang builds huts in the trees.

Although the Orang resides mostly amid the boughs of great trees during the daytime, he is very rarely seen squatting on a thick branch as other apes, and particularly the Gibbons, do. The Orang, on the contrary, confines himself to the slender leafy branches, so that he is seen right at the top

of the trees, a mode of life which is closely related to the constitution of his hinder limbs, and especially to that of his seat. For this is provided with no callosities such as are possessed by many of the lower apes, and even by the Gibbons; and those bones of the pelvis, which are termed the ischia, and which form the solid framework of the surface on which the body rests in the sitting posture, are not expanded like those of the apes which possess callosities, but are more like those of man.

An Orang climbs so slowly and cautiously as, in this act, to resemble a man more than an ape, taking great care of his feet, so that injury of them seems to affect him far more than it does other apes. Unlike the Gibbons, whose forearms do the greater part of the work as they swing from branch to branch, the Orang never makes even the smallest jump. In climbing he moves alternately one hand and one foot, or, after having laid fast hold with the hands, he draws up both feet together. In passing from one tree to another he always seeks out a place where the twigs of both come close together, or interlace. Even when closely pursued, his circumspection is amazing; he shakes the branches to see if they will bear him, and then bending an overhanging bough down by throwing his weight gradually along it, he makes a bridge from the tree he wishes to quit to the next.

On the ground the Orang always goes laboriously and shakily on all fours. At starting he will run faster than a man, though he may soon be overtaken. The very long arms which, when he runs, are but little bent, raise the body of the Orang remarkably, so that he assumes much the posture of a very old man bent down by age, and making his way along by the help of a stick. In walking, the body is usually directed straight forward, unlike the other apes, which run more or less obliquely, except the Gibbons, who in these, as in so many other respects, depart remarkably from their fellows.

The Orang cannot put its feet flat on the ground, but is supported upon their outer edges, the heel resting more on the ground, while the curved toes partly rest upon the ground by the upper side of their first joint, the two outermost toes of each foot completely resting on this surface. The hands are held in the opposite manner, their inner edges serving as the chief support. The fingers are then bent out in such a manner that their foremost joints, especially those of the two innermost fingers, rest upon the ground by their upper sides, while the point of the free and straight thumb serves as an additional fulcrum.

The Orang never stands on its hind legs, and all the pictures representing it as so doing are as false as the assertion that it defends itself with sticks and the like.

The long arms are of especial use, not only in climbing, but in the gathering of food from boughs to which the animal could not trust his weight. Figs, blossoms, and young leaves of various kinds, constitute the chief nutriment of the Orang; but strips of bamboo two or three feet long were found in the stomach of a male. They are not known to eat living animals.

Although, when taken young, the Orang-Utan soon becomes domesticated, and indeed seems to court human society, it is naturally a very wild and shy animal, though apparently sluggish and melancholy. The Dyaks affirm that when the old males are wounded with arrows only they will occasionally leave the trees and rush raging upon their enemies, whose sole safety lies in instant flight, as they are sure to be killed if caught.

But, though possessed of immense strength, it is rare for the Orang to attempt to defend itself, especially when attacked with firearms. On such occasions he endeavors to hide himself, or to escape along the topmost branches of the trees, breaking off and throwing down the boughs as he goes. When wounded he betakes himself to the highest attainable point of the tree, and emits a singular cry, consisting at first of high notes, which at length deepen into a low roar, not unlike that of a panther. While giving out the high notes the Orang thrusts out his lips into a funnel-shape; but in uttering the low notes he holds his mouth wide open, and at the same time the great throat bag, or laryngeal sac, becomes distended.

According to the Dyaks, the only animal the Orang measures his strength with is the crocodile, who occasionally seizes him on his visits to the water-side. But they say that the Orang is more than a match for his enemy, and beats him to death, or rips up his throat by pulling the jaws asunder!

Much of what has been here stated was probably derived by Dr. Müller from the reports of his Dyak hunters; but a large male, four feet high, lived in captivity under his observation for a month, and receives a very bad character.

"He was a very wild beast," says Müller, "of prodigious strength, and false and wicked to the last degree. If any one approached he rose up slowly with a low growl, fixed his eyes in the direction in which he meant to make his attack, slowly passed his hand between the bars of his cage, and then, extending his long arm, gave a sudden grip—usually at the face." He never tried to bite (though Orangs will bite one another), his great weapons of offence and defence being his hands.

His intelligence was very great; and Müller remarks that, though the faculties of the Orang have been estimated too highly, yet Cuvier, had he seen this specimen, would not have considered its intelligence to be only a little higher than that of a dog.

His hearing was very acute, but the sense of vision seemed to be less perfect. The under lip was the great organ of touch, and played a very important part in drinking, being thrust out like a trough, so as either to catch the falling rain or to receive the con-

tents of the half cocoanut shell full of water with which the Orang was supplied, and which, in drinking, he poured into the trough thus formed.

In Borneo the Orang-Utan of the Malays goes by the name of "*Mias*" among the Dyaks, who distinguish several kinds as *Mias Pappan*, or *Zimo*, *Mias Kassu*, and *Mia Rambi*. Whether these are distinct species, however, or whether they are mere races, and how far any of them are identical with the Sumatran Orang, as Mr. Wallace thinks the Mias Pappan to be, are problems which are at present undecided; and the variability of these great apes is so extensive that the settlement of the question is a matter of great difficulty. Of the form called "Mias Pappan," Mr. Wallace observes: "It is known by its large size, and by the lateral expansion of the face into fatty protuberances, or ridges, over the temporal muscles, which have been mistermed *callosities*, as they are perfectly soft, smooth, and flexible. Five of this form, measured by me, varied only from 4 feet 1 inch to 4 feet 2 inches in height, from the heel to the crown of the head, the girth of the body from 3 feet to 3 feet 7½ inches, and the extent of the outstretched arms from 7 feet 2 inches to 7 feet 6 inches; the width of the face from 10 to 13½ inches. The color and length of the hair varied in different individuals, and in different parts of the same individual; some possessed a rudimentary nail on the great toe, others none at all; but they otherwise present no external differences on which to establish even varieties of a species.

"Yet, when we examine the crania of these individuals, we find remarkable differences of form, proportion, and dimension, no two being exactly alike. The slope of the profile, and the projection of the muzzle, together with the size of the cranium, offer differences as decided as those existing between the most strongly marked forms of the Caucasian and African crania in the human species. The orbits vary in width and height, the cranial ridge is either single or double, either much or little developed, and the zygomatic aperture varies considerably in size. This variation in the proportions of the crania enables us satisfactorily to explain the marked difference presented by the single-crested and double-crested skulls, which have been thought to prove the existence of two large species of Orang. The external surface of the skull varies considerably in size, as do also the zygomatic aperture and the temporal muscle; but they bear no necessary relation to each other, a small muscle often existing with a large cranial surface, and *vice versâ*. Now those skulls which have the largest and strongest jaws, and the widest zygomatic aperture, have the muscles so large that they meet on the crown of the skull, and deposit the bony ridge which separates them, and which is the highest in that which has the smallest cranial surface. In those which combine a large surface with comparatively weak jaws, and small zygomatic aperture, the muscles, on each side, do not extend to the crown, a space of from 1 to 2 inches remaining between them, and along their margins small ridges are formed. Intermediate forms are found, in which the ridges meet only in the hinder part of the skull. The form and size of the ridges are therefore independent of age, being sometimes more strongly developed in the less aged animal. Professor Temminck states that the series of skulls in the Leyden Museum shows the same result."

Mr. Wallace observed two male adult Orangs (Mias Kassu of the Dyaks), however, so very different from any of these that he concludes them to be specially distinct; they were respectively 3 feet 8½ inches and 3 feet 9½ inches high, and possessed no sign of the cheek excrescences, but otherwise resembled the larger kinds. The skull has no crest, but two bony ridges, 1½ to 2 inches apart, as in the *Simia morio* of Professor Owen. The teeth, however, are immense, equalling or surpassing those of the other species. The females of both these kinds, according to Mr. Wallace, are devoid of excrescences, and resemble the smaller males, but are shorter by 1½ to 3 inches, and their canine teeth are comparatively small, subtruncated and dilated at the base, as in the so-called *Simia morio*, which is, in all probability, the skull of a female of the same species as the smaller males. Both males and females of this smaller species are distinguishable, according to Mr. Wallace, by the comparatively large size of the middle incisors of the upper jaw.

So far as I am aware, no one has attempted to dispute the accuracy of the statements which I have just quoted regarding the habits of the two Asiatic man-like Apes; and if true, they must be admitted as evidence that such an ape—

1stly. May readily move along the ground in the erect, or semi-erect, position, and without direct support from its arms.

2dly. That it may possess an extremely loud voice—so loud as to be readily heard one or two miles.

3dly. That it may by capable of great viciousness and violence when irritated; and this is especially true of adult males.

4thly. That it may build a nest to sleep in.

Such being well-established facts respecting the Asiatic anthropoids, analogy alone might justify us in expecting the African species to offer similar peculiarities, separately or combined; or, at any rate, would destroy the force of any attempted *à priori* argument against such direct testimony as might be adduced in favor of their existence. And if the organization of any of the African apes could be demonstrated to fit it better than either of its Asiatic allies for the erect position and for efficient attack, there would be still less reason for doubting its occasional adoption of the upright attitude or of aggressive proceedings.

From the time of Tyson and Tulpius

downward the habits of the young Chimpanzee in a state of captivity have been abundantly reported and commented upon. But trustworthy evidence as to the manners and customs of adult anthropoids of this species, in their native woods, was almost wanting up to the time of the publication of the paper by Dr. Savage, to which I have already referred, containing notes of the observations which he made, and of the information which he collected from sources which he considered trustworthy, while resident at Cape Palmas, at the north-western limit of the Bight of Benin.

The adult Chimpanzees, measured by Dr. Savage, never exceeded, though the males may almost attain, five feet in height.

"When at rest, the sitting posture is that generally assumed. They are sometimes seen standing and walking, but when thus detected, they immediately take to all fours and flee from the presence of the observer. Such is their organization that they cannot stand erect, but lean forward. Hence they are seen, when standing, with the hands clasped over the occiput, or the lumbar region, which would seem necessary to balance or ease of posture.

"The toes of the adult are strongly flexed and turned inward, and cannot be perfectly straightened. In the attempt the skin gathers into thick folds on the back, showing that the full expansion of the foot, as is necessary in walking, is unnatural. The natural position is on all fours, the body anteriorly resting upon the knuckles. These are greatly enlarged, with the skin protuberant and thickened like the sole of the foot.

"They are expert climbers, as one would suppose from their organization. In their gambols they swing from limb to limb at a great distance, and leap with astonishing agility. It is not unusual to see the 'old folks' (in the language of an observer) sitting under a tree regaling themselves with fruit and friendly chat, while their 'children' are leaping around them, and swinging from tree to tree with boisterous merriment.

"As seen here, they cannot be called *gregarious*, seldom more than five, or ten at most, being found together. It has been said, on good authority, that they occasionally assemble in large numbers, in gambols. My informant asserts that he saw once not less than fifty so engaged, hooting, screaming, and drumming with sticks upon old logs, which is done in the latter case with equal facility by the four extremities. They do not appear ever to act on the offensive, and seldom, if ever, really on the defensive. When about to be captured, they resist by throwing their arms about their opponent, and attempting to draw him into contact with their teeth."

With respect to this last point Dr. Savage is very explicit in another place:

"*Biting* is their principal art of defence. I have seen one man who had been thus severely wounded in the feet.

"The strong development of the canine teeth in the adult would seem to indicate a carnivorous propensity; but in no state save that of domestication do they manifest it. At first they reject flesh, but easily acquire a fondness for it. The canines are early developed, and evidently designed to act the important part of weapons of defence. When in contact with man almost the first effort of the animal is—*to bite*.

"They avoid the abodes of men, and build their habitations in trees. Their construction is more that of *nests* than *huts*, as they have been erroneously termed by some naturalists. They generally build not far above the ground. Branches or twigs are bent, or partly broken, and crossed, and the whole supported by the body of a limb or a crotch. Sometimes a nest will be found near the *end* of a *strong leafy branch* twenty or thirty feet from the ground. One I have lately seen that could not be less than forty feet, and more probably it was fifty. But this is an unusual height.

"Their dwelling-place is not permanent, but changed in pursuit of food and solitude, according to the force of circumstances. We more often see them in elevated places; but this arises from the fact that the low grounds, being more favorable for the natives' rice-farms, are the oftener cleared, and hence are almost always wanting in suitable trees for their nests. . . . It is seldom that more than one or two nests are seen upon the same tree, or in the same neighborhood: five have been found, but it was an unusual circumstance." . . .

"They are very filthy in their habits. It is a tradition with the natives generally here that they were once members of their own tribe; that for their depraved habits they were expelled from all human society, and that, through an obstinate indulgence of their vile propensities, they have degenerated into their present state of organization. They are, however, eaten by them, and when cooked with the oil and pulp of the palm-nut considered a highly palatable morsel.

"They exhibit a remarkable degree of intelligence in their habits, and, on the part of the mother, much affection for their young. The second female described was upon a tree when first discovered, with her mate and two young ones a (male and a female). Her first impulse was to descend with great rapidity and make off into the thicket with her mate and female offspring. The young male remaining behind, she soon returned to the rescue. She ascended and took him in her arms, at which moment she was shot, the ball passing through the forearm of the young one, on its way to the heart of the mother. . . .

"In a recent case the mother, when discovered, remained upon the tree with her offspring, watching intently the movements of the hunter. As he took aim, she motioned with her hand, precisely in the manner of a human being, to have him desist and go

away. When the wound has not proved instantly fatal, they have been known to stop the flow of blood by pressing with the hand upon the part, and when they did not succeed, to apply leaves and grass. . . . When shot, they give a sudden screech, not unlike that of a human being in sudden and acute distress.

"The ordinary voice of the Chimpanzee, however, is affirmed to be hoarse, guttural, and not very loud, somewhat like 'whoo-whoo.'"

The analogy of the Chimpanzee to the Orang, in its nest-building habit and in the mode of forming its nest, is exceedingly interesting, while, on the other hand, the activity of this ape, and its tendency to bite, are particulars in which it rather resembles the Gibbons. In extent of geographical range, again, the Chimpanzees—which are found from Sierra Leone to Congo—remind one of the Gibbons rather than of either of the other man-like Apes; and it seems not unlikely that, as is the case with the Gibbons, there may be several species spread over the geographical area of the genus.

The same excellent observer, from whom I have borrowed the preceding account of the habits of the adult Chimpanzee, published, fifteen years ago,* an account of the Gorilla, which has, in its most essential points, been confirmed by subsequent observers, and to which so very little has really been added, that, in justice to Dr. Savage, I give it almost in full:

"It should be borne in mind that my account is based upon the statements of the aborigines of that region (the Gaboon). In this connection it may also be proper for me to remark that, having been a missionary resident for several years, studying, from habitual intercourse, the African mind and character, I felt myself prepared to discriminate and decide upon the probability of their statements. Besides, being familiar with the history and habits of its interesting congener (*Trog. niger*, Geoff.), I was able to separate their accounts of the two animals, which, having the same locality and a similarity of habit, are confounded in the minds of the mass, especially as but few—such as traders to the interior, and huntsmen—have ever seen the animal in question.

"The tribe from which our knowledge of the animal is derived, and whose territory forms its habitat, is the *Mpongwe*, occupying both banks of the River Gaboon, from its mouth to some fifty or sixty miles up ward. . . .

"If the word 'Pongo' be of African origin, it is probably a corruption of the word *Mpongwe*, the name of the tribe on the banks of the Gaboon, and hence applied to the region they inhabit. Their local name for the Chimpanzee is *Enché-eko*, as near as it can be Anglicized, from which the common term 'Jocko' probably comes. The Mpongwe appellation for its new congener is *Engé-ena*, prolonging the sound of the first vowel, and slightly sounding the second.

"The habitat of the *Engé-ena* is the interior of Lower Guinea, while that of the *Enché-eko* is nearer the seaboard.

"Its height is about five feet; it is disproportionately broad across the shoulders, thickly covered with coarse black hair, which is said to be similar in its arrangement to that of the *Enché-eko*; with age it becomes gray, which fact has given rise to the report that both animals are seen of different colors.

"*Head.*—The prominent features of the head are the great width and elongation of the face, the depth of the molar region, the branches of the lower jaw being very deep and extending far backward, and the comparative smallness of the cranial portion; the eyes are very large, and said to be like those of the Enché-eko, a bright hazel; nose broad and flat, slightly elevated toward the root; the muzzle broad, and prominent lips and chin, with scattered gray hairs; the under lip highly mobile, and capable of great elongation when the animal is enraged, then hanging over the chin; skin of the face and ears naked, and of a dark-brown, approaching to black.

Fig. 10.—The Gorilla, after Wolf.

"The most remarkable feature of the head is a high ridge, or crest of hair, in the course of the sagittal suture, which meets posteriorly with a transverse ridge of the same, but less prominent, running round from the back of one ear to the other. The animal has the power of moving the scalp freely forward and back, and when enraged is said to contract it strongly over the brow, thus bringing down the hairy ridge and pointing the hair forward, so as to present an indescribably ferocious aspect.

"Neck short, thick, and hairy; chest and shoulders very broad, said to be fully double the size of the Enché-ekos; arms very long, reaching some way below the knee—the forearm much the shortest; hands very large, the thumbs much larger than the fingers. . . .

"The gait is shuffling; the motion of the body, which is never upright as in man, but bent forward, is somewhat rolling, or from side to side. The arms being longer than the Chimpanzee, it does not stoop as much in walking; like that animal, it makes progression by thrusting its arms forward, resting the hands on the ground, and then giving the body a half-jumping half-swinging motion between them. In this act it is said not to flex the fingers, as does the Chimpanzee, resting on its knuckles, but to extend them, making a fulcrum of the hand. When it assumes the walking posture, to which it is said to be much inclined, it balances its huge body by flexing its arms upward.

"They live in bands, but are not so numerous as the Chimpanzees: the females generally exceed the other sex in number. My informants all agree in the assertion that but one adult male is seen in a band; that when the young males grow up a contest takes place for mastery, and the strongest, by killing and driving out the others, establishes himself as the head of the community."

Dr. Savage repudiates the stories about the Gorillas carrying off women and vanquishing elephants, and then adds:

"Their dwellings, if they may be so called, are similar to those of the Chimpanzee, consisting simply of a few sticks and leafy branches, supported by the crotches and limbs of trees; they afford no shelter, and are occupied only at night.

"They are exceedingly ferocious, and always offensive in their habits, never running from man, as does the Chimpanzee. They are objects of terror to the natives, and are never encountered by them except on the defensive. The few that have been captured were killed by elephant-hunters and native traders, as they came suddenly upon them while passing through the forests.

"It is said that when the male is first seen he gives a terrific yell, that resounds far and wide through the forest, something like kh—ah! kh—ah! prolonged and shrill. His enormous jaws are widely opened at each expiration, his under-lip hangs over the chin, and the hairy ridge and scalp are contracted upon the brow, presenting an aspect of indescribable ferocity.

"The females and young, at the first cry, quickly disappear. He then approaches the enemy in great fury, pouring out his horrid cries in quick succession. The hunter awaits his approach with his gun extended: if his aim is not sure he permits the animal to grasp the barrel, and as he carries it to his mouth (which is his habit) he fires. Should the gun fail to go off, the barrel (that of the ordinary musket, which is thin) is crushed between his teeth, and the encounter soon proves fatal to the hunter.

"In the wild state their habits are in general like those of the *Troglodytes niger*, building their nests loosely in trees, living on similar fruits, and changing their place of resort from force of circumstances."

Dr. Savage's observations were confirmed and supplemented by those of Mr. Ford, who communicated an interesting paper on the Gorilla to the Philadelphian Academy of Sciences, in 1852. With respect to the geographical distribution of this greatest of all the man-like Apes, Mr. Ford remarks:

"This animal inhabits the range of mountains that traverse the interior of Guinea from the Cameroon in the north to Angola in the south, and about 100 miles inland, and called by the geographers Crystal Mountains. The limit to which this animal extends, either north or south, I am unable to define. But that limit is doubtless some distance north of this river [Gaboon]. I was able to certify myself of this fact in a late excursion to the head-waters of the Mooney (Danger) River, which comes into the sea some sixty miles from this place. I was informed (credibly, I think), that they were numerous among the mountains in which that river rises, and far north of that.

"In the south, this species extends to the Congo River, as I am told by native traders who have visited the coast, between the Gaboon and that river. Beyond that, I am not informed. This animal is only found at a distance from the coast in most cases, and, according to my best information, approaches it nowhere so nearly as on the south side of this river, where they have been found within ten miles of the sea. This, however, is only of late occurrence. I am informed by some of the oldest Mpongwe men that formerly he was only found on the sources of the river, but that at present he may be found within half a day's walk of its mouth. Formerly he inhabited the mountainous ridge where Bushmen alone inhabited, but now he boldly approaches the Mpongwe plantations. This is doubtless the reason of the scarcity of information in years past, as the opportunities for receiving a knowledge of the animal have not been wanting; traders having for one hundred years frequented this river, and specimens, such as have been brought here within a year, could not have been exhibited without having attracted the attention of the most stupid."

One specimen Mr. Ford examined weighed 170 lbs., without the thoracic or pelvic viscera, and measured four feet four inches round the chest. This writer describes so minutely and graphically the onslaught of the Gorilla—though he does not for a moment pretend to have witnessed the scene—that I am tempted to give this part of his paper in full, for comparison with other narratives:

"He always rises to his feet when making an attack, though he approaches his antagonist in a stooping posture.

"Though he never lies in wait, yet, when he hears, sees, or scents a man, he immediately utters his characteristic cry, prepares for an attack, and always acts on the offensive. The cry he utters resembles a grunt more than a growl, and is similar to the cry of the Chimpanzee when irritated, but vastly louder. It is said to be audible at a great distance. His preparation consists in attending the females and young ones, by whom he is usually accompanied to a little distance. He, however, soon returns, with his crest erect and projecting forward, his nostrils dilated, and his under-lip thrown down; at the same time uttering his characteristic yell, designed, it would seem, to terrify his antagonist. Instantly, unless he is disabled by a well-directed shot, he makes an onset, and, striking his antagonist with the palm of his hands, or seizing him with a grasp from which there is no escape, he dashes him upon the ground, and lacerates him with his tusks.

"He is said to seize a musket, and instantly crush the barrel between his teeth. . . This animal's savage nature is very well shown by the implacable desperation of a young one that was brought here. It was taken very young, and kept four months, and many means were used to tame it; but it was incorrigible, so that it bit me an hour before it died."

Mr. Ford discredits the house-building and elephant-driving stories, and says that no well-informed natives believe them. They are tales told to children.

I might quote other testimony to a similar effect, but, as it appears to me, less carefully weighed and sifted, from the letters of MM. Franquet and Gautier Laboullay, appended to the memoir of M. I. G. St. Hilaire, which I have already cited.

Bearing in mind what is known regarding the Orang and the Gibbon, the statements of Dr. Savage and Mr. Ford do not appear to me to be justly open to criticism on *à priori* grounds. The Gibbons, as we have seen, readily assume the erect posture, but the Gorilla is far better fitted by its organization for that attitude than are the Gibbons: if the laryngeal pouches of the Gibbons, as is very likely, are important in giving volume to a voice which can be heard for half a league, the Gorilla, which has similar sacs, more largely developed, and whose bulk is fivefold that of a Gibbon, may well be audible for twice that distance.

If the Orang fights with its hands, the Gibbons and Chimpanzees with their teeth, the Gorilla may, probably enough, do either or both; nor is there anything to be said against either Chimpanzee or Gorilla building a nest, when it is proved that the Orang Utan habitually performs that feat.

With all this evidence, now ten to fifteen years old, before the world, it is not a little surprising that the assertions of a recent traveller, who, so far as the Gorilla is concerned, really does very little more than repeat, on his own authority, the statements of Savage and Ford, should have met with so much and such bitter opposition. If subtraction be made of what was known before, the sum and substance of what M. Du Chaillu has affirmed as a matter of his own observation respecting the Gorilla, is that, on advancing to the attack, the great brute beats his chest with his fists. I confess I see nothing very improbable, or very much worth disputing about, in this statement.

With respect to the other man-like Apes of Africa, M. Du Chaillu tells us absolutely nothing, of his own knowledge, regarding the common Chimpanzee; but he informs us of a bald-headed species or variety, the *nschiego mbouve*, which builds itself a shelter, and of another rare kind with a comparatively small face, large facial angle, and peculiar note, resembling "Kooloo."

As the Orang shelters itself with a rough coverlet of leaves, and the common Chimpanzee, according to that eminently trustworthy observer Dr. Savage, makes a sound like "Whoo-whoo"—the grounds of the summary repudiation with which M. Du Chaillu's statements on these matters have been met is not obvious.

If I have abstained from quoting M. Du Chaillu's work, then, it is not because I discern any inherent improbability in his assertions respecting the man-like Apes; nor from any wish to throw suspicion on his veracity; but because, in my opinion, so long as his narrative remains in its present state of unexplained and apparently inexplicable confusion, it has no claim to original authority respecting any subject whatsoever.

It may be truth, but it is not evidence.

AFRICAN CANNIBALISM IN THE SIXTEENTH CENTURY.

In turning over Pigafetta's version of the narrative of Lopez, which I have quoted above, I came upon so curious and unexpected an anticipation, by some two centuries and a half, of one of the most startling parts of M. Du Chaillu's narrative, that I cannot refrain from drawing attention to it in a note, although I must confess that the subject is not strictly relevant to the matter in hand.

In the fifth chapter of the first book of the "Descriptio," concerning the northern part of the kingdom of Congo and its boundaries, is mentioned a people whose king is called "Maniloango," and who live under the equator, and as far westward as Cape Lopez. This appears to be the country now inhabited by the Ogobai and Bakalai according to M. Du Chaillu. "Beyond these dwell another people called 'Anziques,' of incredible ferocity, for they eat one another, sparing neither friends nor relations."

These people are armed with small bows bound tightly round with snake-skins, and strung with a reed or rush. Their arrows

short and slender, but made of hard wood, are shot with great rapidity. They have iron axes, the handles of which are bound with snake-skins, and swords with scabbards of the same material; for defensive armor they employ elephant-hides. They cut their skins when young, so as to produce scars. "Their butchers' shops are filled with human flesh instead of that of oxen or sheep. For they eat the enemies whom they take in battle. They fatten, slay, and devour their slaves, also, unless they think they shall get a good price for them; and, moreover, sometimes for weariness of life or desire for glory (for they think it a great thing and the sign of a generous soul to despise life), or for love of their rulers, offer themselves up for food.

"There are indeed many cannibals, as in the Eastern Indies and in Brazil and elsewhere, but none such as these, since the others only eat their enemies, but these their own blood relations."

FIG. 11.—Butcher's Shop of the Anziques, Anno 1598

The careful illustrators of Pigafetta have done their best to enable the reader to realize this account of the "Anziques," and the unexampled butcher's shop represented in Fig. 11, is a fac-simile of part of their Plate XII.

M. Du Chaillu's account of the Fans accords most singularly with what Lopez here narrates of the Anziques. He speaks of their small crossbows and little arrows, of their axes and knives, "ingeniously sheathed in snake-skins." "They tattoo themselves more than any other tribes I have met with north of the equator." And all the world knows what M. Du Chaillu says of their cannibalism: "Presently we passed a woman who solved all doubt. She bore with her a piece of the thigh of a human body, just as we should go to market and carry thence a roast or steak." M. Du Chaillu's artist cannot generally be accused of any want of courage in embodying the statements of his author, and it is to be regretted that, with so good an excuse, he has not furnished us with a fitting companion to the sketch of the brothers De Bry.

II.

ON THE RELATIONS OF MAN TO THE LOWER ANIMALS.

To many it might appear that there is a greater difference between Monkey and Man than between day and night. Yet, on comparing the highest type of Europeans with the Hottentots who live at the Cape of Good Hope, they will, with difficulty, convince themselves that both are of one origin. Or if they would compare a highly polished and cultured Court Lady, with a savage thrown upon his own resources, they could hardly imagine that he and she belong to the same species.—*Linnæus, Amœnitates Academicæ* "*Anthropomorpha.*"

THE question of questions for mankind—the problem which underlies all others, and is more deeply interesting than any other—is the ascertainment of the place which Man occupies in nature and of his relations to the universe of things. Whence our race has come; what are the limits of our power over nature, and of nature's power over us; to what goal we are tending; are the problems which present themselves anew and with undiminished interest to every man born into the world. Most of us, shrinking from the difficulties and dangers which beset the seeker after original answers to these riddles, are contented to ignore them altogether, or to smother the investigating spirit under the feather-bed of respected and respectable tradition. But, in every age, one or two restless spirits, blessed with that constructive genius, which can only build on a secure foundation, or cursed with the mere spirit of scepticism, are unable to follow in the well-worn and comfortable track of their forefathers and contemporaries, and, unmindful of thorns and stumbling-blocks, strike out into paths of their own. The sceptics end in the infidelity which asserts the problem to be insoluble, or in the atheism which denies the existence of any orderly progress and governance of things: the men of genius propound solutions which grow into systems of theology or of philosophy, or veiled in mu-

sical language which suggests more than it asserts, take the shape of the poetry of an epoch.

Each such answer to the great question, invariably asserted by the followers of its propounder, if not by himself, to be complete and final, remains in high authority and esteem, it may be for one century, or it may be for twenty; but, as invariably, time proves each reply to have been a mere approximation to the truth—tolerable chiefly on account of the ignorance of those by whom it was accepted, and wholly intolerable when tested by the larger knowledge of their successors.

In a well-worn metaphor, a parallel is drawn between the life of man and the metamorphosis of the caterpillar into the butterfly; but the comparison may be more just as well as more novel, if for its former term we take the mental progress of the race. History shows that the human mind, fed by constant accessions of knowledge, periodically grows too large for its theoretical coverings, and bursts them asunder to appear in new habiliments, as the feeding and growing grub, at intervals, casts its too narrow skin and assumes another, itself but temporary. Truly the imago state of Man seems to be terribly distant, but every moult is a step gained, and of such there have been many.

Since the revival of learning, whereby the Western races of Europe were enabled to enter upon that progress toward true knowledge which was commenced by the philosophers of Greece, but was almost arrested in subsequent long ages of intellectual stagnation, or, at most, gyration, the human larva has been feeding vigorously, and moulting in proportion. A skin of some dimension was cast in the sixteenth century, and another toward the end of the eighteenth, while, within the last fifty years, the extraordinary growth of every department of physical science has spread among us mental food of so nutritious and stimulating a character that a new ecdysis seems imminent. But this is a process not unusually accompanied by many throes and some sickness and debility, or, it may be, by graver disturbances; so that every good citizen must feel bound to facilitate the process, and even if he have nothing but a scalpel to work withal, to ease the cracking integument to the best of his ability.

In this duty lies my excuse for the publication of these essays. For it will be admitted that some knowledge of man's position in the animate world is an indispensable preliminary to the proper understanding of his relations to the universe—and this again resolves itself, in the long run, into an inquiry into the nature and the closeness of the ties which connect him with those singular creatures whose history has been sketched in the preceding pages.

The importance of such an inquiry is indeed intuitively manifest. Brought face to face with these blurred copies of himself, the least thoughtful of men is conscious of a certain shock, due, perhaps, not so much to disgust at the aspect of what looks like an insulting caricature, as to the awakening of a sudden and profound mistrust of time-honored theories and strongly-rooted prejudices regarding his own position in nature, and his relations to the under-world of life; while that which remains a dim suspicion for the unthinking, becomes a vast argument, fraught with the deepest consequences, for all who are acquainted with the recent progress of the anatomical and physiological sciences.

I now propose briefly to unfold that argument, and to set forth, in a form intelligible to those who possess no special acquaintance with anatomical science, the chief facts upon which all conclusions respecting the nature and the extent of the bonds which connect man with the brute world must be based: I shall then indicate the one immediate conclusion which, in my judgment, is justified by these facts, and I shall finally discuss the bearing of that conclusion upon the hypotheses which have been entertained respecting the origin of man.

The facts to which I would first direct the reader's attention, though ignored by many of the professed instructors of the public mind, are easy of demonstration and are universally agreed to by men of science; while their significance is so great that whoso has duly pondered over them will, I think, find little to startle him in the other revelations of biology. I refer to those facts which have been made known by the study of development.

It is a truth of very wide, if not of universal, application, that every living creature commences its existence under a form different from and simpler than that which it eventually attains.

The oak is a more complex thing than the little rudimentary plant contained in the acorn; the caterpillar is more complex than the egg; the butterfly than the caterpillar; and each of these beings, in passing from its rudimentary to its perfect condition, runs through a series of changes, the sum of which is called its development. In the higher animals these changes are extremely complicated; but within the last half century the labors of such men as Von Baer, Rathke, Reichert, Bischoff, and Remak, have almost completely unravelled them, so that the successive stages of development which are exhibited by a dog, for example, are now as well known to the embryologist as are the steps of the metamorphosis of the silkworm moth to the school-boy. It will be useful to consider with attention the nature and the order of the stages of canine development, as an example of the process in the higher animals generally.

The dog, like all animals save the very lowest (and further inquiries may not improbably remove the apparent exception), commences its existence as an egg: as a body

which is, in every sense, as much an egg as that of a hen, but is devoid of that accumulation of nutritive matter which confers upon the bird's egg its exceptional size and domestic utility; and wants the shell, which would not only be useless to an animal incubated within the body of its parent, but would cut it off from access to the source of that nutriment which the young creature requires, but which the minute egg of the mammal does not contain within itself.

The dog's egg is, in fact, a little spheroidal bag (Fig. 12) formed of a delicate transparent membrane called the *vitelline membrane*, and about $\frac{1}{100}$ to $\frac{1}{120}$th of an inch in diameter. It contains a mass of viscid nutritive matter—the "*yelk*"—within which is inclosed a second much more delicate spheroidal bag, called the "*germinal vesicle*" (*a*). In this, lastly, lies a more solid rounded body, termed the "*germinal spot*" (*b*).

The egg, or "Ovum," is originally formed within a gland, from which, in due season,

FIG. 12.—A. Egg of the Dog, with the vitelline membrane burst, so as to give exit to the yelk, the germinal vesicle (*a*), and its included spot (*b*). B. C. D. E. F. Successive changes of the yelk indicated in the text. After Bischoff.

it becomes detached, and passes into the living chamber fitted for its protection and maintenance during the protracted process of gestation. Here, when subjected to the required conditions, this minute and apparently insignificant particle of living matter becomes animated by a new and mysterious activity. The germinal vesicle and spot cease to be discernible (their precise fate being one of the yet unsolved problems of embryology), but the yelk becomes circumferentially indented, as if an invisible knife had been drawn round it, and thus appears divided into two hemispheres (Fig. 12, C).

By the repetition of this process in various planes, these hemispheres become subdivided, so that four segments are produced (D); and these, in like manner, divide and subdivide again, until the whole yelk is converted into a mass of granules, each of which consists of a minute spheroid of yelk-substance, inclosing a central particle, the so-called "*nucleus*" (F). Nature, by this process, has attained much the same result as that at which a human artificer arrives by his operations in a brick-field. She takes the rough plastic material of the yelk and breaks it up into well-shaped tolerably even-sized masses—handy for building up into any part of the living edifice.

Next, the mass of organic bricks, or "*cells*" as they are technically called, thus formed, acquires an orderly arrangement, becoming converted into a hollow spheroid with double walls. Then, upon one side of this spheroid appears a thickening, and, by and by, in the centre of the area of thickening, a straight shallow groove (Fig. 13, A) marks the central line of the edifice which is to be raised, or, in other words, indicates the position of the middle line of the body of the future dog. The substance bounding the groove on each side next rises up into a fold, the rudiment of the side wall of that long cavity, which will eventually lodge the spinal marrow and the brain; and in the floor of this chamber appears a solid cellular cord, the so-called "*notochord*." One end of the inclosed cavity dilates to form the head (Fig. 13, B), the other remains narrow, and eventually becomes the tail; the side walls of the body are fashioned out of the downward continuation of the walls of the groove; and from them, by and by, grow out little buds which, by degrees, assume the shape of limbs. Watching the fashioning process stage by stage, one is forcibly reminded of the modeller in clay. Every part, every organ, is at first, as it were, pinched up rudely, and sketched out in the rough; then shaped more accurately, and only, at last, receives the touches which stamp its final character.

Thus, at length, the young puppy assumes such a form as is shown in Fig. 13, C. In this condition it has a disproportionately large head, as dissimilar to that of a dog as the bud-like limbs are unlike his legs.

The remains of the yelk, which have not yet been applied to the nutrition and growth of the young animal, are contained in a sac attached to the rudimentary intestine, and termed the yelk sac, or "*umbilical vesicle*." Two membranous bags, intended to subserve respectively the protection and nutrition of

the young creature, have been developed from the skin and from the under and hinder surface of the body; the former, the so-called "*amnion*," is a sac filled with fluid, which invests the whole body of the embryo, and plays the part of a sort of water-bed for it; the other, termed the "*allantois*," grows out, loaded with blood-vessels, from the ventral region, and eventually applying itself to the walls of the cavity, in which the developing organism is contained, enables these vessels to become the channel by which the stream of nutriment, required to supply the wants of the offspring, is furnished to it by the parent.

The structure which is developed by the interlacement of the vessels of the offspring with those of the parent and by means of

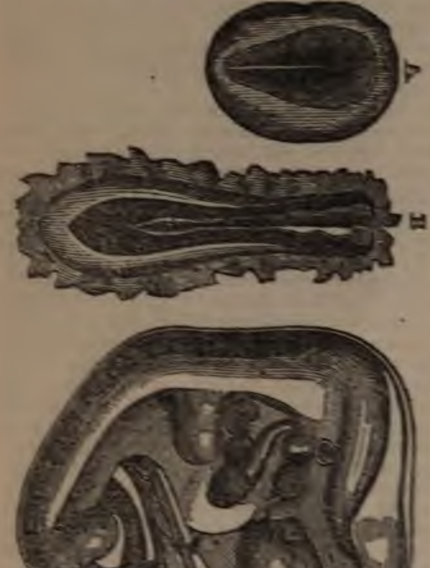


FIG. 18.—A. Earliest rudiment of the dog. B. Rudiment further advanced, showing the foundations of the head, tail, and vertebral column. C. The very young puppy, with attached ends of the yelk sac and allantois, and invested in the amnion.

which the former is enabled to receive nourishment and to get rid of effete matters, is termed the "*Placenta*."

It would be tedious, and it is unnecessary for my present purpose, to trace the process of development further: suffice it to say that, by a long and gradual series of changes, the rudiment here depicted and described becomes a puppy, is born, and then, by still slower and less perceptible steps, passes into the adult dog.

There is not much apparent resemblance between a barn-door fowl and the dog who protects the farm-yard. Nevertheless the student of development finds, not only that the chick commences its existence as an egg, primarily identical, in all essential respects, with that of the dog, but that the yelk of this egg undergoes division—that the primitive groove arises, and that the contiguous parts of the germ are fashioned, by precisely similar methods, into a young chick, which, at one stage of its existence, is so like the nascent dog that ordinary inspection would hardly distinguish the two.

The history of the development of any other vertebrate animal, lizard, snake, frog, or fish, tells the same story. There is always, to begin with, an egg having the same essential structure as that of the dog—the yelk of that egg always undergoes division, or "*segmentation*" as it is often called; the ultimate products of that segmentation constitute the building materials for the body of the young animal; and this is built up round a primitive groove, in the floor of which a notochord is developed. Furthermore, there is a period in which the young of all these animals resemble one another, not merely in outward form, but in all essentials of structure, so closely that the differences between them are inconsiderable, while in their subsequent course they diverge more and more widely from one another. And it is a general law that, the more closely any animals resemble one another in adult structure, the longer and the more intimately do their embryos resemble one another; so that, for example, the embryos of a snake and of a lizard remain like one another longer than do those of a snake and of a bird; and the embryo of a dog and of a cat remain like one another for a far longer period than do those of a dog and a bird; or of a dog and an opossum; or even than those of a dog and a monkey.

Thus the study of development affords a clear test of closeness of structural affinity, and one turns with impatience to inquire what results are yielded by the study of the development of man. Is he something apart? Does he originate in a totally different way from dog, bird, frog, and fish, thus justifying those who assert him to have no place in nature and no real affinity with the lower world of animal life? Or does he originate in a similar germ, pass through the same slow and gradually progressive modifications—depend on the same contrivances for protection and nutrition, and finally enter the world by the help of the same mechanism? The reply is not doubtful for a moment, and has not been doubtful any time these thirty years. Without question, the mode of origin and the early stages of the development of man are identical with those of the animals immediately below him in the scale; without a doubt, in these respects, he is far nearer the apes than the apes are to the dog.

The human ovum is about $\frac{1}{125}$ of an inch

in diameter, and might be described in the same terms as that of the dog, so that I need only refer to the figure illustrative (14 A) of its structure. It leaves the organ in which it is formed in a similar fashion and enters the organic chamber prepared for its reception in the same way, the conditions of its development being in all respects the same. It has not yet been possible (and only by some rare chance can it ever be possible) to study the human ovum in so early a developmental stage as that of yelk division, but there is every reason to conclude that the changes it undergoes are identical with those exhibited by the ova of other vertebrated animals; for the formative materials of which the rudimentary human body is composed, in the earliest conditions in which it has been observed, are the same as those of other animals. Some of these earliest stages are figured below, and, as will be seen, they are strictly comparable to the very early states of the dog; the marvellous correspondence between the two which is kept up, even for some time, as development advances, becoming apparent by the simple comparison of the figures with others previously shown.

Indeed, it is very long before the body of the young human being can be readily discriminated from that of the young puppy; but, at a tolerably early period, the two become distinguishable by the different form of their adjuncts, the yelk sac and the allantois. The former, in the dog, becomes long and spindle-shaped, while in man it remains spherical; the latter, in the dog, attains an extremely large size, and the vascular processes which are developed from it and eventually give rise to the formation of the placenta (taking root, as it were, in the parental organism, so as to draw nourishment therefrom, as the root of a tree extracts it from the soil) are arranged in an encircling zone, while in man the allantois remains comparatively small, and its vascular rootlets are eventually restricted to one disk-like spot. Hence, while the placenta of the dog is like a girdle, that of man has the cake-like form indicated by the name of the organ.

But exactly in those respects in which the developing man differs from the dog, he resembles the ape, which, like man, has a spheroidal yelk-sac and a discoidal—sometimes partially lobed placenta.

So that it is only quite in the later stages of development that the young human being presents marked differences from the young ape, while the latter departs as much from the dog in its development as the man does.

Startling as the last assertion may appear to be, it is demonstrably true, and it alone appears to me sufficient to place beyond all doubt the structural unity of man with the rest of the animal world, and more particularly and closely with the apes.

Thus, identical in the physical processes by which he originates—identical in the early stages of his formation—identical in the mode of his nutrition before and after birth, with the animals which lie immediately below him in the scale—man, if his adult and perfect structure be compared with theirs, exhibits, as might be expected, a marvellous likeness of organization. He resembles them as they resemble one another—he differs from them as they differ from one another. And, though these differences and resemblances cannot be weighed and measured, their value may be readily estimated; the scale or standard of judgment, touching that value, being afforded and expressed by the system of classification of animals now current among zoologists.

A careful study of the resemblances and differences presented by animals has, in fact, led naturalists to arrange them into groups or assemblages, all the members of each group presenting a certain amount of definable resemblance, and the number of points of similarity being smaller as the group is larger and *vicê versâ*. Thus all creatures which agree only in presenting the few distinctive marks of animality form the "Kingdom" Animalia. The numerous animals which agree only in possessing the special characters of vertebrates form one "sub-kingdom" of this

FIG. 14.—A. Human ovum (after Kölliker). *a.* Germinal vesicle; *b.* germinal spot. B. A very early condition of Man, with yelk-sac, allantois and amnion (original). C. A more advanced stage (after Kölliker), compare fig. 13, C.

kingdom. Then the sub kingdom vertebrata is subdivided into the five "classes," fishes, amphibians, reptiles, birds, and mammals, and these into smaller groups called "orders;" these into "families" and "genera;" while the last are finally broken up into the smallest assemblages, which are distinguished by the possession of constant, not-sexual, characters. These ultimate groups are Species.

Every year tends to bring about a greater uniformity of opinion throughout the zoological world as to the limits and characters of these groups, great and small. At present, for example, no one has the least doubt regarding the characters of the classes mammalia, aves, or reptilia; nor does the question arise whether any thoroughly well-known animal should be placed in one class or the other. Again there is a very general agreement respecting the characters and limits of the orders of mammals, and as to the animals which are structurally necessitated to take a place in one or another order.

No one doubts, for example, that the Sloth and the Ant-eater, the Kangaroo and the Opossum, the Tiger and the Badger, the Tapir and the Rhinoceros, are respectively members of the same orders. These successive pairs of animals may and some do differ from one another immensely, in such matters as the proportions and structure of their limbs; the number of their dorsal and lumbar vertebræ; the adaptation of their frames to climbing, leaping, or running; the number and form of their teeth; and the characters of their skulls and of the contained brain. But, with all these differences, they are so closely connected in all the more important and fundamental characters of their organization, and so distinctly separated by these same characters from other animals, that zoologists find it necessary to group them together as members of one order. And if any new animal were discovered, and were found to present no greater difference from the Kangaroo and the Opossum, for example, than these animals do from one another, the zoologist would not only be logically compelled to rank it in the same order with these, but he would not think of doing otherwise.

Bearing this obvious course of zoological reasoning in mind, let us endeavor for a moment to disconnect our thinking selves from the mask of humanity; let us imagine ourselves scientific Saturnians, if you will, fairly acquainted with such animals as now inhabit the earth, and employed in discussing the relations they bear to a new and singular "erect and featherless biped," which some enterprising traveller, overcoming the difficulties of space and gravitation, has brought from that distant planet for our inspection, well preserved, may be, in a cask of rum. We should all, at once, agree upon placing him among the mammalian vertebrates; and his lower jaw, his molars, and his brain, would leave no room for doubting the systematic position of the new genus among those mammals, whose young are nourished during gestation by means of a placenta, or what are called the "placental mammals."

Further, the most superficial study would at once convince us that among the orders of placental mammals, neither the Whales nor the hoofed creatures, nor the Sloths and Ant-eaters, nor the carniverous Cats, Dogs, and Bears, still less the Rodent Rats and Rabbits, or the Insectivorous Moles and Hedgehogs, or the Bats, could claim our "*Homo*" as one of themselves.

There would remain, then, but one order for comparison, that of the Apes (using that word in its broadest sense), and the question for discussion would narrow itself to this—is Man so different from any of these Apes that he must form an order by himself? Or does he differ less from them than they differ from one another, and hence must take his place in the same order with them?

Being happily free from all real or imaginary personal interest in the results of the inquiry thus set afoot, we should proceed to weigh the arguments on one side and on the other, with as much judicial calmness as if the question related to a new Opossum. We should endeavor to ascertain, without seeking either to magnify or diminish them, all the characters by which our new Mammal differed from the Apes; and if we found that these were of less structural value than those which distinguish certain members of the Ape order from others universally admitted to be of the same order, we should undoubtedly place the newly discovered tellurian genus with them.

I now proceed to detail the facts which seem to me to leave us no choice but to adopt the last-mentioned course.

It is quite certain that the Ape which most nearly approaches man, in the totality of its organization, is either the Chimpanzee or the Gorilla; and as it makes no practical difference, for the purposes of my present argument, which is selected for comparison, on the one hand, with Man, and on the other hand, with the rest of the Primates, I shall select the latter (so far as its organization is known)—as a brute now so celebrated in prose and verse, that all must have heard of him, and have formed some conception of his appearance. I shall take up as many of the most important points of difference between man and this remarkable creature as the space at my disposal will allow me to discuss, and the necessities of the argument demand; and I shall inquire into the value and magnitude of these differences, when placed side by side with those which separate the Gorilla from other animals of the same order.

In the general proportions of the body and limbs there is a remarkable difference between the Gorilla and Man, which at once strikes the eye. The Gorilla's brain-case is smaller, its trunk larger, its lower limbs shorter its upper limbs longer in proportion

than those of Man

I find that the vertebral column of a full-grown Gorilla, in the Museum of the Royal College of Surgeons, measures 27 inches along its anterior curvature, from the upper edge of the atlas, or first vertebra of the neck, to the lower extremity of the sacrum; that the arm, without the hand, is $31\frac{1}{2}$ inches long; that the leg, without the foot, is $26\frac{1}{2}$ inches long: that the hand is $9\frac{3}{4}$ inches long; the foot $11\frac{1}{4}$ inches long.

In other words, taking the length of the spinal column as 100, the arm equals 115, the leg 96, the hand 36, and the foot 41.

In the skeleton of a male Bosjesman, in the same collection, the proportions, by the same measurement, to the spinal column, taken as 100, are—the arm 78, the leg 110, the hand 26, and the foot 32. In a woman of the same race the arm is 83, and the leg 120, the hand and foot remaining the same. In a European skeleton I find the arm to be 80, the leg 117, the hand 26, the foot 35.

Thus the leg is not so different as it looks at first sight, in its proportions to the spine in the Gorilla and in the Man—being very slightly shorter than the spine in the former, and between $\frac{1}{10}$ and $\frac{1}{8}$ longer than the spine in the latter. The foot is longer and the hand much longer in the Gorilla; but the great difference is caused by the arms, which are very much longer than the spine in the Gorilla, very much shorter than the spine in the man.

The question now arises, how are the other Apes related to the Gorilla in these respects—taking the length of the spine, measured in the same way, at 100. In an adult Chimpanzee the arm is only 96, the leg 90, the hand 43, the foot 39—so that the hand and the leg depart more from the human proportion and the arm less, while the foot is about the same as in the Gorilla.

In the Orang, the arms are very much longer than in the Gorilla (122), while the legs are shorter (88); the foot is longer than the hand (52 and 48), and both are much longer in proportion to the spine.

In the other man-like Apes, again, the Gibbons, these proportions are still further altered, the length of the arms being to that of the spinal column as 19 to 11, while the legs are also a third longer than the spinal column, so as to be longer than in Man, instead of shorter. The hand is half as long as the spinal column, and the foot, shorter than the hand, is about $\frac{4}{11}$ of the length of the spinal column.

Thus *Hylobates* is as much longer in the arms than the Gorilla as the Gorilla is longer in the arms than Man; while, on the other hand, it is as much longer in the legs than the Man as the Man is longer in the legs than the Gorilla, so that it contains within itself the extremest deviations from the average length of both pairs of limbs (see the Frontispiece).

The Mandrill presents a middle condition, the arms and legs being nearly equal in length, and both being shorter than the spinal column; while hand and foot have nearly the same proportions to one another and to the spine, as in Man.

In the Spider monkey (*Ateles*) the leg is longer than the spine, and the arm than the leg; and, finally, in that remarkable Lemurine form, the Indri (*Lichanotus*), the leg is about as long as the spinal column, while the arm is not more than $\frac{11}{18}$ of its length, the hand having rather less and the foot rather more than one third the length of the spinal column.

These examples might be greatly multiplied, but they suffice to show that, in whatever proportion of its limbs the Gorilla differs from Man, the other Apes depart still more widely from the Gorilla, and that, consequently, such differences of proportion can have no ordinal value.

We may next consider the differences presented by the trunk, consisting of the vertebral column, or backbone, and the ribs and pelvis, or bony hip-basin, which are connected with it, in Man and in the Gorilla respectively.

In Man, in consequence partly of the disposition of the articular surfaces of the vertebræ, and largely of the elastic tension of some of the fibrous bands, or ligaments, which connect these vertebræ together, the spinal column, as a whole, has an elegant S-like curvature, being convex forward in the neck, concave in the back, convex in the loins, or lumbar region, and concave again in the sacral region; an arrangement which gives much elasticity to the whole backbone, and diminishes the jar communicated to the spine, and through it to the head, by locomotion in the erect position.

Furthermore, under ordinary circumstances, Man has seven vertebræ in his neck, which are called *cervical*; twelve succeed these, bearing ribs and forming the upper part of the back, whence they are termed *dorsal*; five lie in the loins, bearing no distinct, or free, ribs, and are called *lumbar*; five, united together into a great bone, excavated in front, solidly wedged in between the hip bones, to form the back of the pelvis, and known by the name of the *sacrum*, succeed these; and finally, three or four little more or less movable bones, so small as to be insignificant, constitute the *coccyx* or rudimentary tail.

In the Gorilla the vertebral column is similarly divided into cervical, dorsal, lumbar, sacral, and coccygeal vertebræ, and the total number of cervical and dorsal vertebræ, taken together, is the same as in man; but the development of a pair of ribs to the first lumbar vertebra, which is an exceptional occurrence in Man, is the rule in the Gorilla; and hence, as lumbar are distinguished from dorsal vertebræ only by the presence or absence of free ribs, the seventeen "dorso-lumbar" vertebræ of the Gorilla are divided into thirteen dorsal and four lumbar, while in Man they are twelve dorsal and five lumbar.

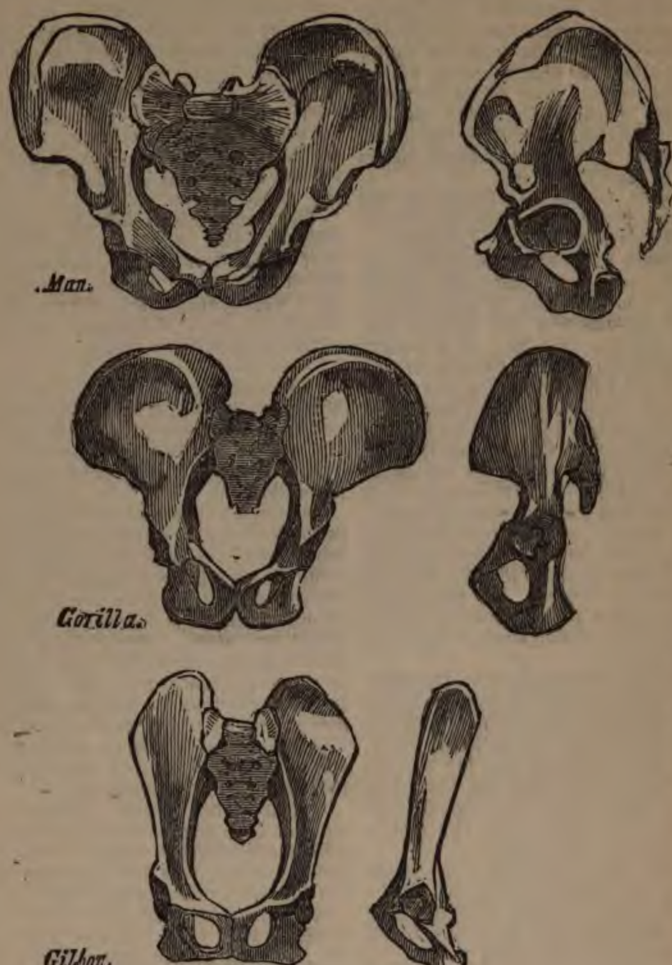


FIG. 15.—Front and side views of the bony pelvis of Man, the Gorilla and Gibbon: reduced from drawings made from nature, of the same absolute length, by Mr. Waterhouse Hawkins.

Not only, however, does Man occasionally possess thirteen pair of ribs, but the Gorilla sometimes has fourteen pairs, while an Orang-Utan skeleton in the Museum of the Royal College of Surgeons has twelve dorsal and five lumbar vertebræ, as in Man. Cuvier notes the same number in a *Hylobates*. On the other hand, among the lower Apes, many possess twelve dorsal and six or seven lumbar vertebræ; the Douroucouli has fourteen dorsal and eight lumbar, and a Lemur (*Stenops tardigradus*) has fifteen dorsal and nine lumbar vertebræ.

The vertebral column of the Gorilla, as a whole, differs from that of Man in the less marked character of its curves, especially in the slighter convexity of the lumbar region. Nevertheless, the curves are present, and are quite obvious in young skeletons of the Gorilla and Chimpanzee which have been prepared without removal of the ligaments. In young Orangs similarly preserved, on the other hand, the spinal column is either straight, or even concave forward, throughout the lumbar region.

Whether we take these characters then, or such minor ones as those which are derivable from the proportional length of the spines in the cervical vertebræ, and the like, there is no doubt whatsoever as to the marked difference between Man and the Gorilla: but there is as little, that equally marked differences, of the very same order, obtain between the Gorilla and the lower apes.

The pelvis, or bony girdle of the hips, of Man is a strikingly human part of his organi-

zation; the expanded haunch-bones affording support for his viscera during his habitually erect posture, and giving space for the attachment of the great muscles which enable him to assume and to preserve that attitude. In these respects the pelvis of the Gorilla differs very considerably from his (Fig. 15). But go no lower than the Gibbon, and see how vastly more he differs from the Gorilla than the latter does from Man, even in this structure. Look at the flat, narrow haunch-bones—the long and narrow passage—the coarse, outwardly curved, ischiatic prominences on which the Gibbon habitually rests, and which are coated by the so-called callosities," dense patches of skin, wholly absent in the Gorilla, in the Chimpanzee, and in the Orang, as in Man!

In the lower Monkeys and in the Lemurs the difference becomes more striking still, the pelvis acquiring an altogether quadrupedal character.

But now let us turn to a nobler and more characteristic organ—that by which the human frame seems to be, and indeed is, so strongly distinguished from all others—I mean the skull. The differences between a Gorilla's skull and a Man's are truly immense (Fig. 16). In the former, the face, formed largely by the massive jaw-bones, predominates over the brain-case, or cranium proper: in the latter, the proportions of the two are reversed. In the Man, the occipital foramen, through which passes the great nervous cord connecting the brain with the nerves of the body, is placed just behind the centre of the base of the skull, which thus becomes evenly balanced in the erect posture; in the Gorilla it lies in the posterior third of that base. In the Man, the surface of the skull is comparatively smooth, and the supraciliary ridges or brow prominences usually project but little—while, in the Gorilla, vast crests are developed upon the skull, and the brow ridges overhang the cavernous orbits, like great penthouses.

Sections of the skulls, however, show that some of the apparent defects of the Gorilla's cranium arise, in fact, not so much from deficiency of brain-case as from excessive development of the parts of the face. The cranial cavity is not ill-shaped, and the forehead is not truly flattened or very retreating, its really well-formed curve being simply disguised by the mass of bone which is built up against it (Fig. 16).

But the roofs of the orbits rise more obliquely into the cranial cavity, thus diminishing the space for the lower part of the anterior lobes of the brain, and the absolute capacity of the cranium is far less than that of Man. So far as I am aware, no human cranium belonging to an adult man has yet been observed with a less cubical capacity than 62 cubic inches, the smallest cranium observed in any race of men by Morton, measuring 63 cubic inches; while, on the other hand, the most capacious Gorilla skull yet measured has a content of not more than $34\frac{1}{2}$ cubic inches. Let us assume, for simplicity's sake, that the lowest Man's skull has twice the capacity of the highest Gorilla.

No doubt this is a very striking difference, but it loses much of its apparent systematic value when viewed by the light of certain other equally indubitable facts respecting cranial capacities.

The first of these is, that the difference in the volume of the cranial cavity of different races of mankind is far greater, absolutely, than that between the lowest Man and the highest Ape, while, relatively, it is about the same. For the largest human skull measured by Morton contained 114 cubic inches—that is to say, had very nearly double the capacity of the smallest; while its absolute preponderance of 52 cubic inches is far greater than that by which the lowest adult male human cranium surpasses the largest of the Gorillas ($62 - 34\frac{1}{2} = 27\frac{1}{2}$). Secondly, the adult crania of Gorillas which have as yet been measured differ among themselves by nearly one third, the maximum capacity being 34.5 cubic inches, the minimum 24 cubic inches; and, thirdly, after making all due allowance for difference of size, the cranial capacities of some of the lower apes fall nearly as much, relatively, below those of the higher Apes as the latter fall below Man.

Thus, even in the important matter of cranial capacity, Men differ more widely from one another than they do from the Apes, while the lowest Apes differ as much, in proportion, from the highest, as the latter does from Man. The last proposition is still better illustrated by the study of the modifications which other parts of the cranium undergo in the Simian series.

It is the large proportional size of the facial bones and the great projection of the jaws which confers upon the Gorilla's skull its small facial angle and brutal character.

But if we consider the proportional size of the facial bones to the skull proper only, the little *Chrysothrix* (Fig. 16) differs very widely from the Gorilla, and in the same way as Man does; while the Baboons (*Cynocephalus*, Fig. 16) exaggerate the gross proportions of the muzzle of the great Anthropoid, so that its visage looks mild and human by comparison with theirs. The difference between the Gorilla and the Baboon is even greater than it appears at first sight; for the great facial mass of the former is largely due to a downward development of the jaws; an essentially human character, superadded upon that almost purely forward, essentially brutal, development of the same parts which characterizes the Baboon, and yet more remarkably distinguishes the Lemur.

Similarly the occipital foramen of *Mycetes* (Fig. 16), and still more of the Lemurs, is situated completely in the posterior face of the skull, or as much farther back than that of the Gorilla as that of the Gorilla is further back than that of Man; while, as if to render patent the futility of the attempt to

base any broad classificatory distinction on such a character, the same group of Platyrhine, or American monkeys, to which the *Mycetes* belongs, contains the *Chrysothrix*, whose occipital foramen is situated far more forward than in any other ape, and nearly approaches the position it holds in Man.

Again, the Orang's skull is as devoid of extensively developed supraciliary prominences as a man's, though some varieties exhibit great crests elsewhere (see p. [illegible]); and in some of the Cebine apes and in the *Chrysothrix*, the cranium is as smooth and rounded as that of Man himself.

What is true of these leading characteristics of the skull holds good, as may be imagined, of all minor features; so that for every constant difference between the Gorilla's skull and the Man's a similar constant difference of the same order (that is to say, consisting in excess or defect of the same quality) may be found between the Gorilla's skull and that of some other ape. So that, for the skull, no less than for the skeleton in

THE RELATIONS OF MAN

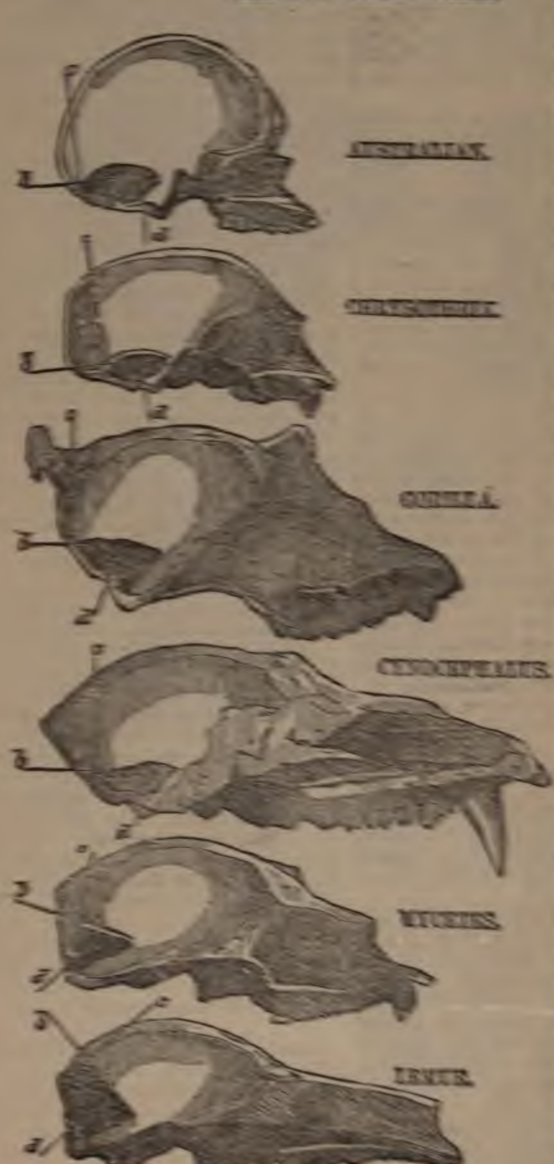


FIG. 16.—Sections of the skulls of Man and various Apes, drawn so as to give the cerebral cavity the same length in each case, thereby displaying the varying proportions of the facial bones. The line *b* indicates the plane of the tentorium, which separates the cerebrum from the cerebellum; *d*, the axis of the occipital outlet of the skull. The extent of the cerebral cavity behind *c*, which is a perpendicular erected on *b* at the point where the tentorium is attached posteriorly, indicates the degree to which the cerebrum overlaps the cerebellum—the space occupied by which is roughly indicated by the dark shading. In comparing these diagrams, it must be recollected, that figures on so small a scale as these simply exemplify the statements in the text, the proof of which is to be found in the objects themselves.

general, the proposition holds good that the differences between Man and the Gorilla are of smaller value than those between the Gorilla and some other Apes.

In connection with the skull, I may speak of the teeth—organs which have a peculiar classificatory value, and whose resemblances and differences of number, form, and succession, taken as a whole, are usually regarded as more trustworthy indicators of affinity than any others.

Man is provided with two sets of teeth—milk teeth and permanent teeth. The former consist of four incisors, or cutting-teeth; two canines, or eye-teeth; and four molars, or grinders, in each jaw, making twenty in all. The latter (Fig. 17) comprise four incisors, two canines, four small grinders, called premolars or false molars, and six large grinders, or true molars in each jaw—making thirty-two in all. The internal incisors are larger than the external pair, in the upper jaw, smaller than the external pair, in the lower jaw. The crowns of the upper molars exhibit four cusps, or blunt-pointed elevations, and a ridge crosses the crown obliquely, from the inner, anterior, cusp to the outer, posterior cusp (Fig. 17 m^2). The anterior lower molars have five cusps, three external and two internal. The premolars have two cusps, one internal and one external, of which the outer is the higher.

In all these respects the dentition of the Gorilla may be described in the same terms as that of Man; but in other matters it exhibits many and important differences (Fig. 17).

Thus the teeth of man constitute a regular and even series—without any break and without any marked projection of one tooth above the level of the rest; a peculiarity which, as

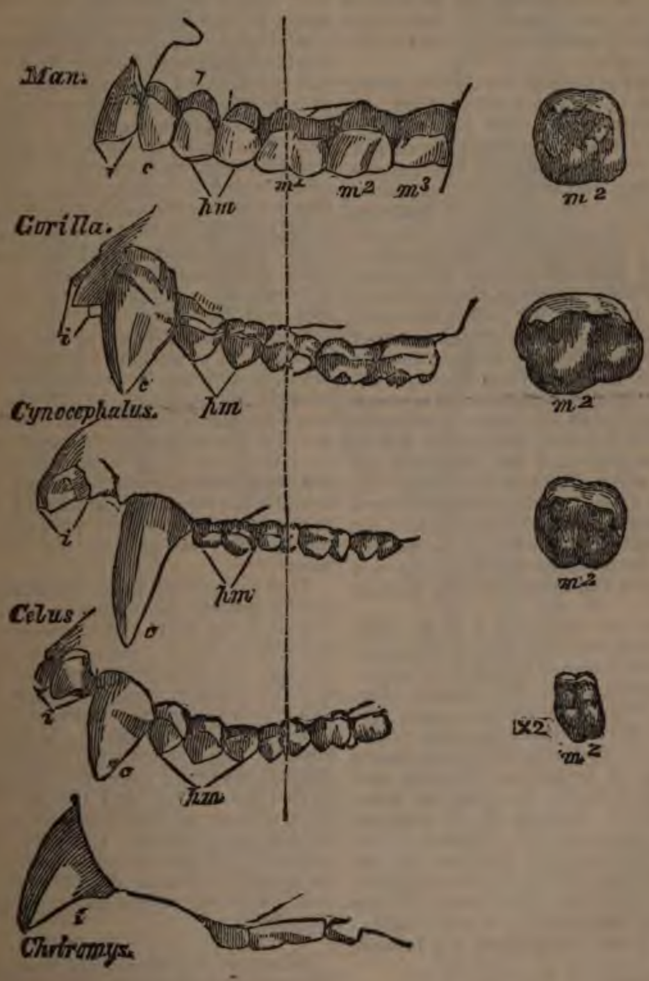


FIG. 17.—Lateral views, of the same length, of the upper jaws of various Primates. *i*, incisors; *c*, canines; *pm*. premolars; *m*, molars. A line is drawn through the first molar of Man, *Gorilla*, *Cynocephalus*, and *Cebus*, and the grinding surface of the second molar is shown in each, its anterior and internal angle being just above the *m* of m^2.

Cuvier long ago showed, is shared by no other mammal save one—as different a creature from man as can well be imagined—namely, the long extinct *Anoplotherium*. The teeth of the Gorilla, on the contrary, exhibit a break, or interval, termed the *diastema*, in both jaws: in front of the eye-tooth, or between it and the outer incisor, in the upper jaw; behind the eye-tooth, or between it and the front false molar in the lower jaw. Into this break in the series, in each jaw, fits the canine of the opposite jaw, the size of the eye-tooth in the Gorilla being so great that it projects, like a tusk, far beyond the general level of the other teeth. The roots of the false molar teeth of the Gorilla, again, are more complex than in Man, and the proportional size of the molars is different. The Gorilla has the crown of the hindmost grinder of the lower jaw more complex, and the order of eruption of the permanent teeth is different, the permanent canines making their appearance before the second and third molars in Man, and after them in the Gorilla.

Thus, while the teeth of the Gorilla closely resemble those of Man in number, kind, and in the general pattern of their crowns, they exhibit marked differences from those of Man in secondary respects, such as relative size, number of fangs, and order of appearance.

But if the teeth of the Gorilla be compared with those of an Ape, no further removed from it than a *Cynocephalus*, or Baboon, it will be found that differences and resemblances of the same order are easily observable; but that many of the points in which the Gorilla resembles Man are those in which it differs from the Baboon, while various respects in which it differs from Man are exaggerated in the *Cynocephalus*. The number and the nature of the teeth remain the same in the Baboon as in the Gorilla and in Man. But the pattern of the Baboon's upper molars is quiet different from that described above (Fig. 17), the canines are proportionally longer and more knife-like; the anterior premolar in the lower jaw is specially modified; the posterior molar of the lower jaw is still larger and more complex than in the Gorilla.

Passing from the old-world Apes to those of the new world, we meet with a change of much greater importance than any of these. In such a genus as *Cebus*, for example (Fig. 17), it will be found that while in some secondary points, such as the projection of the canines and the diastema, the resemblance to the great ape is preserved; in other and most important respects, the dentition is extremely different. Instead of 20 teeth in the milk set, there are 24; instead of 32 teeth in the permanent set, there are 36, the false molars being increased from eight to twelve. And in form the crowns of the molars are very unlike those of the Gorilla, and differ far more widely from the human pattern.

The Marmosets, on the other hand, exhibit the same number of teeth as Man and the Gorilla; but, notwithstanding this, their dentition is very different, for they have four more false molars, like the other American monkeys—but as they have four fewer true molars, the total remains the same. And passing from the American apes to the Lemurs, the dentition becomes still more completely and essentially different from that of the Gorilla. The incisors begin to vary both in number and in form. The molars acquire, more and more, a many-pointed, insectivorous character, and in one Genus, the Aye-Aye (*Cheiromys*), the canines disappear, and the teeth completely simulate those of a Rodent (Fig. 17).

Hence it is obvious that, greatly as the dentition of the highest Ape differs from that of Man, it differs far more widely from that of the lower and lowest Apes.

Whatever part of the animal fabric—whatever series of muscles, whatever viscera might be selected for comparison—the result would be the same—the lower Apes and the Gorilla would differ more than the Gorilla and the Man. I cannot attempt in this place to follow out all these comparisons in detail, and indeed it is unnecessary I should do so. But certain real, or supposed, structural distinctions between man and the apes remain, upon which so much stress has been laid that they require careful consideration in order that the true value may be assigned to those which are real, and the emptiness of those which are fictitious may be exposed. I refer to the characters of the hand, the foot, and the brain.

Man has been defined as the only animal possessed of two hands terminating his fore limbs, and of two feet ending his hind limbs, while it has been said that all the apes possess four hands; and he has been affirmed to differ fundamentally from all the apes in the characters of his brain, which alone, it has been strangely asserted and reasserted, exhibits the structures known to anatomists as the posterior lobe, the posterior cornu of the lateral ventricle and the hippocampus minor.

That the former proposition should have gained general acceptance is not surprising—indeed, at first sight, appearances are much in its favor; but, as for the second, one can only admire the surpassing courage of its enunciator, seeing that it is an innovation which is not only opposed to generally and justly accepted doctrines, but which is directly negatived by the testimony of all original inquirers, who have specially investigated the matter; and that it neither has been, nor can be, supported by a single anatomical preparation. It would, in fact, be unworthy of serious refutation, except for the general and natural belief that deliberate and reiterated assertions must have some foundation.

Before we can discuss the first point with advantage we must consider with some attention, and compare together, the structure

of the human hand and that of the human foot, so that we may have distinct and clear ideas of what constitutes a hand and what a foot.

The external form of the human hand is familiar enough to every one. It consists of a stout wrist followed by a broad palm, formed of flesh, and tendons, and skin, binding together four bones, and dividing into four long and flexible digits, or fingers, each of which bears on the back of its last joint a broad and flattened nail. The longest cleft between any two digits is rather less than half as long as the hand. From the outer side of the base of the palm a stout digit goes off, having only two joints instead of three; so short that it only reaches to a little beyond the middle of the first joint of the finger next it; and further remarkable by its great mobility, in consequence of which it can be directed outward, almost at a right angle to the rest. This digit is called the "*pollex*," or thumb; and, like the others, it bears a flat nail, upon the back of its terminal joint. In consequence of the proportions and mobility of the thumb, it is what is termed "opposable:" in other words, its extremity can, with the greatest ease, be brought into contact with the extremities of any of the fingers; a property upon which the possibility of our carrying into effect the conceptions of the mind so largely depends.

The external form of the foot differs widely from that of the hand; and yet, when closely compared, the two present some singular resemblances. Thus the ankle corresponds in a maner with the wrist; the sole with the palm; the toes with the fingers; the great toe with the thumb. But the toes, or digits of the foot, are far shorter in proportion than the digits of the hand, and are less movable, the want of mobility being most striking in the great toe—which, again, is very much larger in proportion to the other toes than the thumb to the fingers. In considering this point, however, it must not be forgotten that the civilized great toe, confined and cramped from childhood upward, is seen to a great disadvantage, and that in uncivilized and barefooted people it retains a great amount of mobility, and even some sort of opposability. The Chinese boatmen are said to be able to pull an oar; the artisans of Bengal to weave, and the Carajas to steal fish-hooks by its help; though, after all, it must be recollected that the structure of its joints and the arrangement of its bones necessarily render its prehensile action far less perfect than that of the thumb.

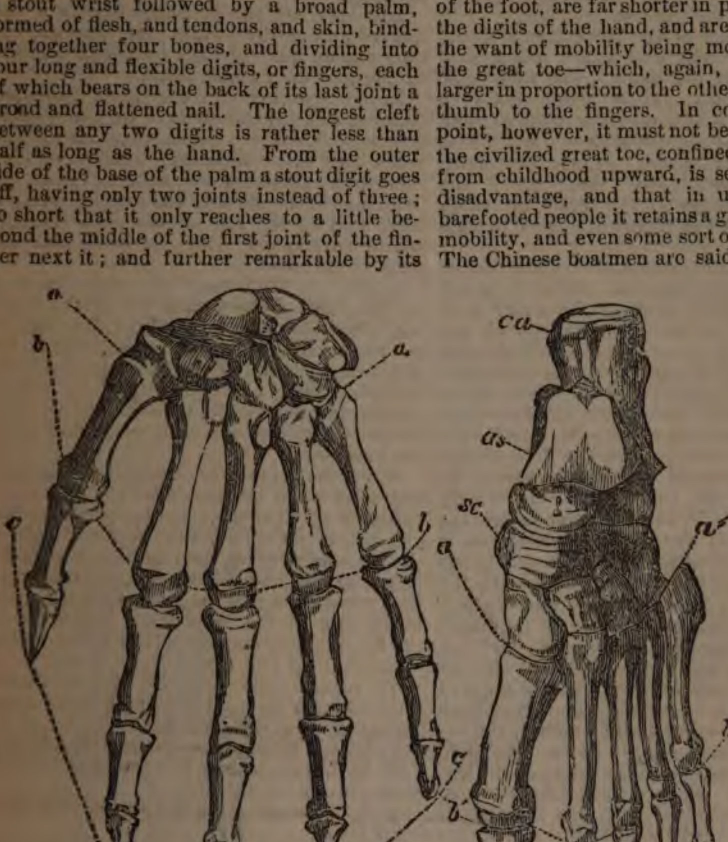


FIG. 18.—The skeleton of the Hand and Foot of Man reduced from Dr. Carter's drawings in Gray's "Anatomy." The hand is drawn to a larger scale than the foot. The line *a a* in the hand indicates the boundary between the carpus and the metacarpus; *b b* that between the latter and the proximal phalanges; *c c* marks the ends of the distal phalanges. The line *a' a'* in the foot indicates the boundary between the tarsus and the metatarsus; *b' b'* marks that between the metatarsus and the proximal phalanges; and *c' c'* bounds the ends of the distal phalanges; *ca*, the calcaneum; *as*, the astragalus; *sc*, the scaphoid bone in the tarsus.

But to gain a precise conception of the resemblances and differences of the hand and foot, and of the distinctive characters of each, we must look below the skin, and compare the bony framework and its motor apparatus in each (Fig. 18).

The skeleton of the hand exhibits, in the

and foot diverge still more from those of the Gorilla than they do in the Orang. The thumb ceases to be opposable in the American monkeys; is reduced to a mere rudiment covered by the skin in the Spider Monkey; and is directed forward and armed with a curved claw like the other digits in the Marmosets—so that in all these cases there can be no doubt but that the hand is more different from that of the Gorilla than the Gorilla's hand is from Man's.

And as to the foot, the great toe of the Marmoset is still more insignificant in proportion than that of the Orang—while in the Lemurs it is very large, and as completely thumb-like and opposable as in the Gorilla—but in these animals the second toe is often irregularly modified, and in some species the two principal bones of the tarsus, the *astragalus* and the *os calcis*, are so immensely elongated as to render the foot so far totally unlike that of any other mammal.

So with regard to the muscles. The short flexor of the toes of the Gorilla differs from that of Man by the circumstance that one slip of the muscle is attached, not to the heel-bone but to the tendons of the long flexors. The lower Apes depart from the Gorilla by an exaggeration of the same character, two, three, or more, slips becoming fixed to the long flexor tendons—or by a multiplication of the slips. Again, the Gorilla differs slightly from Man in the mode of interlacing of the long flexor tendons; and the lower apes differ from the Gorilla in exhibiting yet other, sometimes very complex, arrangements of the same parts, and occasionally in the absence of the accessory fleshy bundle.

Throughout all these modifications it must be recollected that the foot loses no one of its essential characters. Every Monkey and Lemur exhibits the characteristic arrangement of tarsal bones, possesses a short flexor and short extensor muscle, and a *peronæus longus*. Varied as the proportions and appearance of the organ may be, the terminal division of the hind limb remains, in plan and principle of construction, a foot, and never, in those respects, can be confounded with a hand.

Hardly any part of the bodily frame, then, could be found better calculated to illustrate the truth that the structural differences between Man and the highest Ape are of less value than those between the highest and the lower Apes, than the hand or the foot, and yet, perhaps, there is one organ the study of which enforces the same conclusion in a still more striking manner—and that is the Brain.

But before entering upon the precise question of the amount of difference between the Ape's brain and that of Man, it is necessary that we should clearly understand what constitutes a great, and what a small, difference in cerebral structure; and we shall be best enabled to do this by a brief study of the chief modifications which the brain exhibits in the series of vertebrate animals.

The brain of a fish is very small, compared with the spinal cord into which it is continued, and with the nerves which come off from it; of the segments of which it is composed—the olfactory lobes, the cerebral hemisphere, and the succeeding divisions—no one predominates so much over the rest as to obscure or cover them; and the so-called optic lobes are frequently the largest masses of all. In Reptiles the mass of the brain, relatively to the spinal cord, increases, and the cerebral hemispheres begin to predominate over the other parts, while in Birds this predominance is still more marked. The brain of the lowest Mammals, such as the duck-billed Platypus and the Opossums and Kangaroos, exhibits a still more definite advance in the same direction. The cerebral hemispheres have now so much increased in size as, more or less, to hide the representatives of the optic lobes, which remain comparatively small, so that the brain of a Marsupial is extremely different from that of a Bird, Reptile, or Fish. A step higher in the scale, among the placental Mammals, the structure of the brain acquires a vast modification—not that it appears much altered externally, in a Rat or in a Rabbit, from what it is in a Marsupial—nor that the proportions of its parts are much changed, but an apparently new structure is found between the cerebral hemispheres, connecting them together, as what is called the "great commissure" or "corpus callosum." The subject requires careful reinvestigation, but if the currently received statements are correct, the appearance of the "corpus callosum" in the placental mammals is the greatest and most sudden modification exhibited by the brain in the whole series of vertebrated animals—it is the greatest leap anywhere made by Nature in her brain work. For the two halves of the brain being once thus knit together, the progress of cerebral complexity is traceable through a complete series of steps from the lowest Rodent, or Insectivore, to Man; and that complexity consists chiefly in the disproportionate development of the cerebral hemispheres and of the cerebellum, but especially of the former, in respect to the other parts of the brain.

In the lower placental mammals the cerebral hemispheres leave the proper upper and posterior face of the cerebellum completely visible, when the brain is viewed from above, but, in the higher forms, the hinder part of each hemisphere, separated only by the tentorium from the anterior face of the cerebellum, inclines backward and downward, and grows out, as the so-called "posterior lobe," so as at length to overlap and hide the cerebellum. In all Mammals each cerebral hemisphere contains a cavity which is termed the "ventricle," and as this ventricle is prolonged, on the one hand, forward, and on the other downward, into the substance of the hemisphere, it is said to have two horns or "cornua," an "anterior cornu" and a "descending cornu." When the posterior lobe is well developed, a third prolongation of the ventricular cavity extends

into it, and is called the "posterior cornu."

In the lower and smaller forms of placental Mammals the surface of the cerebral hemispheres is either smooth or evenly rounded, or exhibits a very few grooves, which are technically termed "sulci," separating ridges or "convolutions" of the substance of the brain; and the smaller species of all orders tend to a similar smoothness of brain. But in the higher orders, and especially the larger members of these orders, the grooves, or sulci, become extremely numerous, and the intermediate convolutions proportionately more complicated in their meanderings, until, in the Elephant, the Porpoise, the higher Apes, and Man, the cerebral surface appears a perfect labyrinth of tortuous foldings.

Where a posterior lobe exists and presents its customary cavity—the posterior cornu—it commonly happens that a particular sulcus appears upon the inner and under surface of the lobe, parallel with and beneath the floor of the cornu—which is, as it were, arched over the roof of the sulcus. It is as if the groove had been formed by indenting the floor of the posterior horn from without with a blunt instrument, so that the floor should rise as a convex eminence. Now this eminence is what has been termed the "Hippocampus minor," the "Hippocampus major" being a larger eminence in the floor of the descending cornu. What may be the functional importance of either of these structures we know not.

As if to demonstrate, by a striking example, the impossibility of erecting any cerebral barrier between man and the apes, nature has provided us, in the latter animals, with an almost complete series of gradations from brains little higher than that of a Rodent to brains little lower than that of Man. And it is a remarkable circumstance that though, so far as our present knowledge extends, there *is* one true structural break in the series of forms of Simian brains, this hiatus does not lie between Man and the man-like apes, but between the lower and the lowest Simians; or, in other words, between the old and new world apes and monkeys and the Lemurs. Every Lemur which has yet been examined, in fact, has its cerebellum partially visible from above, and its posterior lobe, with the contained posterior cornu and hippocampus minor, more or less rudimentary. Every Marmoset, American monkey, old world monkey, Baboon, or Man-like ape

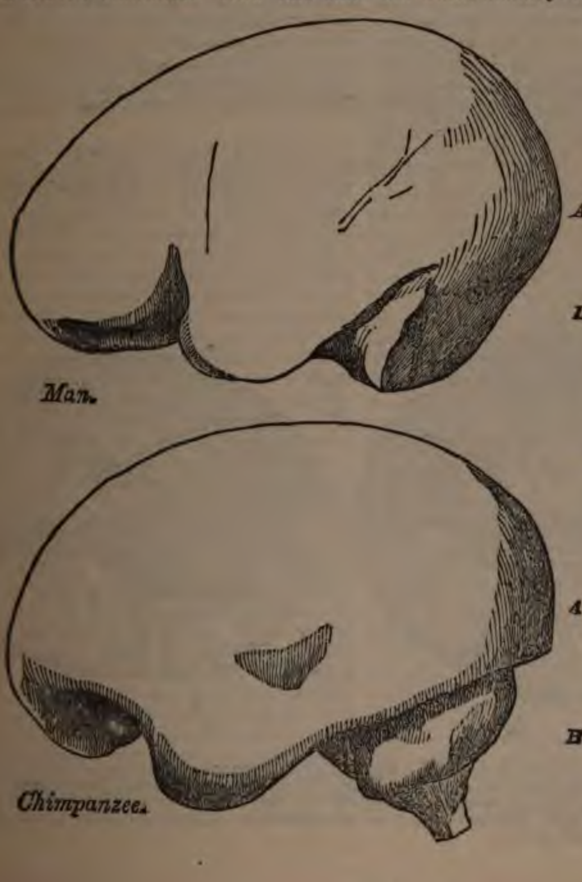


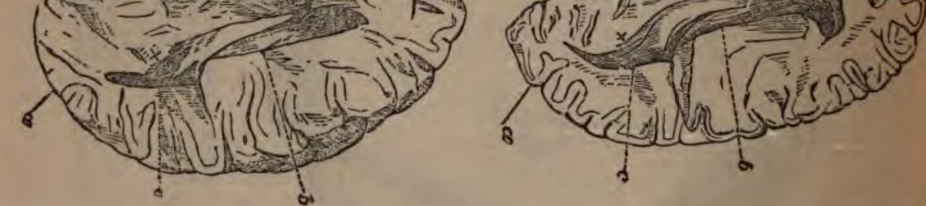
FIG. 20.—Drawings of the internal casts of a Man's and of a Chimpanzee's skull, of the same absolute length, and placed in corresponding positions. *A.* Cerebrum; *B.* Cerebellum. The former drawing is taken from a cast in the Museum of the Royal College of Surgeons, the latter from the photograph of the cast of a Chimpanzee's skull, which illustrates the paper by Mr. Marshall, "On the Brain of the Chimpanzee," in the *Natural History Review* for July, 1861. The sharper definition of the lower edge of the cast of the cerebral chamber in the Chimpanzee arises from the circumstance that the tentorium remained in that skull and not in the Man's. The cast more accurately represents the brain in the Chimpanzee than in Man; and the great backward projection of the posterior lobes of the cerebrum of the former, beyond the cerebellum, is conspicuous.

on the contrary, has its cerebellum entirely hidden, posteriorly, by the cerebral lobes, and possesses a large posterior cornu, with a well-developed hippocampus minor.

In many of these creatures, such as the Saimiri (*Chrysothrix*), the cerebral lobes overlap and extend much farther behind the cerebellum, in proportion, than they do in man (Fig. 16); and it is quite certain that, in all, the cerebellum is completely covered behind by well-developed posterior lobes. The fact can be verified by every one who possesses the skull of any old or new world monkey. For, inasmuch as the brain in all mammals completely fills the cranial cavity, it is obvious that a cast of the interior of the skull will reproduce the general form of the brain, at any rate with such minute and, for the present purpose, utterly unimportant differences as may result from the absence of the enveloping membranes of the brain in the dry skull. But if such a cast be made in plaster, and compared with a similar cast of the interior of a human skull, it will be obvious that the cast of the cerebral chamber, representing the cerebrum of the ape, as completely covers over and overlaps the cast of the cerebellar chamber, representing the cerebellum, as it does in the man (Fig. 20). A careless observer, forgetting that a soft structure like the brain loses its proper shape the moment it is taken out of the skull, may indeed mistake the uncovered condition of the cerebellum of an extracted and distorted brain for the natural relations of the parts; but his error must become patent even to himself if he try to replace the brain within the cranial chamber. To suppose that the cerebellum of an ape is naturally uncovered behind is a miscomprehension comparable only to that of one who should imagine that a man's lungs always occupy but a small portion of the thoracic cavity—because they do so when the chest is opened, and their elasticity is no longer neutralized by the pressure of the air.

And the error is the less excusable, as it must become apparent to every one who examines a section of the skull of any ape above a Lemur, without taking the trouble to make a cast of it. For there is a very marked groove in every such skull, as in the human skull—which indicates the line of attachment of what is termed the *tentorium*—a sort of parchment-like shelf, or partition, which, in the recent state, is interposed between the cerebrum and the cerebellum, and prevents the former from pressing upon the latter (see Fig. 16).

This groove, therefore, indicates the line of separation between that part of the cranial cavity which contains the cerebrum and that which contains the cerebellum; and as the brain exactly fills the cavity of the skull, it is obvious that the relations of these two parts of the cranial cavity at once informs us of the relations of their contents. Now in man, in all the old world and in all the new world Simiæ, with one exception, when the face is directed forward this line of attach-



21.—Drawings of the cerebral hemispheres of a Man and of a Chimpanzee of the same length, in order to show [the] proportions of the parts; the former taken from a specimen, which Mr. Flower, Conservator of the Museum [of the Ro]yal College of Surgeons, was good enough to dissect for me; the latter, from the photograph of a similarly dis[sected Ch]impanzee's brain, given in Mr. Marshall's paper above referred to. *a*, posterior lobe; *b*, lateral ventricle; *c* [posterior] cornu; *x*, the hippocampus minor.

ment of the tentorium, or impression for the lateral sinus, as it is technically called, is nearly horizontal, and the cerebral chamber invariably overlaps or projects behind the cerebellar chamber. In the Howler Monkey, or *Mycetes* (see Fig. 16), the line passes obliquely upward and backward, and the cerebral overlap is almost nil; while in the Lemurs, as in the lower mammals, the line is much more inclined in the same direction, and the cerebellar chamber projects considerably beyond the cerebral.

When the gravest errors respecting points so easily settled as this question respecting the posterior lobes can be authoritatively propounded, it is no wonder that matters of observation of no very complex character, but still requiring a certain amount of care, should have fared worse. Any one who cannot see the posterior lobe in an ape's brain is not likely to give a very valuable opinion respecting the posterior cornu or the hippocampus minor. If a man cannot see a church, it is preposterous to take his opinion about its altar-piece or painted window—so that I do not feel bound to enter upon any discussion of these points, but content myself with assuring the reader that the posterior cornu and the hippocampus minor have now been seen—usually, at least as well developed as in man, and often better—not only in the Chimpanzee, the Orang, and the Gibbon, but in all the genera of the old world baboons and monkeys, and in most of the new world forms, including the Marmosets.

In fact, all the abundant and trustworthy evidence (consisting of the results of careful investigations directed to the determination of these very questions by skilled anatomists) which we now possess leads to the conviction that, so far from the posterior lobe, the posterior cornu, and the hippocampus minor, being structures peculiar to and characteristic of man, as they have been over and over again asserted to be, even after the publication of the clearest demonstration of the reverse, it is precisely these structures which are the most marked cerebral characters common to man with the apes. They are among the most distinctly Simian peculiarities which the human organism exhibits.

As to the convolutions, the brains of the apes exhibit every stage of progress, from the almost smooth brain of the Marmoset, to the Orang and the Chimpanzee, which fall but little below Man. And it is most remarkable that, as soon as all the principal sulci appear, the pattern according to which they are arranged is identical with that of the corresponding sulci of man. The surface of the brain of a monkey exhibits a sort of skeleton map of man's, and in the man-like apes the details become more and more filled in, until it is only in minor characters, such as the greater excavation of the anterior lobes, the constant presence of fissures usually absent in man, and the different disposition and proportions of some convolutions, that the Chimpanzee's or the Orang's brain can be structurally distinguished from Man's.

So far as cerebral structure goes, therefore, it is clear that Man differs less from the Chimpanzee or the Orang than these do even from the Monkeys, and that the difference between the brains of the Chimpanzee and of Man is almost insignificant, when compared with that between the Chimpanzee brain and that of a Lemur.

It must not be overlooked, however, that there is a very striking difference in absolute mass and weight between the lowest human brain and that of the highest ape—a difference which is all the more remarkable when we recollect that a full-grown Gorilla is probably pretty nearly twice as heavy as a Bosjes man, or as many an European woman. It may be doubted whether a healthy human adult brain ever weighed less than thirty one or two ounces, or that the heaviest Gorilla brain has exceeded twenty ounces.

This is a very noteworthy circumstance, and doubtless will one day help to furnish an explanation of the great gulf which intervenes between the lowest man and the highest ape in intellectual power; * but it has lit-

* I say *help* to furnish, for I by no means believe that it was any original difference of cerebral quality, or quantity, which caused that divergence between the human and the pithecoid stirpes, which has ended in the present enormous gulf between them. It is no doubt perfectly true, in a certain sense, that all difference of function is a result of difference of structure; or in other words, of difference in the combination of the primary molecular forces of living substance; and, starting from this undeniable axiom, objectors occasionally, and with much seeming plausibility, argue that the vast intellectual chasm between the Ape and Man implies a corresponding structural chasm in the organs of the intellectual functions, so that, it is said, the non-discovery of such vast differences proves, not that they are absent, but that Science is incompetent to detect them. A very little consideration, however, will, I think, show the fallacy of this reasoning. Its validity hangs upon the assumption that intellectual power depends altogether on the brain, whereas the brain is only one condition out of many on which intellectual manifestations depend, the others being chiefly the organs of the senses and the motor apparatuses, especially those which are concerned in prehension and in the production of articulate speech.

A man born dumb, notwithstanding his great cerebral mass and his inheritance of strong intellectual instincts, would be capable of few higher intellectual manifestations than an Orang or a Chimpanzee, if he were confined to the society of dumb associates. And yet there might not be the slightest discernible difference between his brain and that of a highly intelligent and cultivated person. The dumbness might be the result of a defective structure of the mouth, or of the tongue, or a mere defective innervation of these parts; or it might result from congenital deafness, caused by some minute defect of the internal ear, which only a careful anatomist could discover.

The argument, that because there is an immense difference between a Man's intelligence and an Ape's, therefore there must be an equally immense difference between their brains, appears to me to be about as well based as the reasoning by which one should endeavor to prove that, because there is a "great gulf" between a watch that keeps accurate time and another that will not go at all, there is therefore a great structural hiatus between the two watches. A hair in the balance-wheel, a little rust on a pinion, a bend in a tooth of the escapement, a something so slight that only the practised eye of the watchmaker can discover it, may be the source of all the difference.

tle systematic value, for the simple reason that, as may be concluded from what has been already said respecting cranial capacity, the difference in weight of brain between the highest and the lowest men is far greater, both relatively and absolutely, than that between the lowest man and the highest ape. The latter, as has been seen, is represented by, say, twelve ounces of cerebral substance absolutely, or by 32 : 20 relatively ; but as the largest recorded human brain weighed between 65 and 66 ounces, the former difference is represented by more than 33 ounces absolutely, or by 65 : 32 relatively. Regarded systematically, the cerebral differences of man and apes are not of more than generic value, his family distinction resting chiefly on his dentition, his pelvis, and his lower limbs.

Thus, whatever system of organs be studied, the comparison of their modifications in the ape series leads to one and the same result—that the structural differences which separate Man from the Gorilla and the Chimpanzee are not so great as those which separate the Gorilla from the lower apes.

But in enunciating this important truth I must guard myself against a form of misunderstanding which is very prevalent. I find, in fact, that those who endeavor to teach what nature so clearly shows us in this matter are liable to have their opinions misrepresented and their phraseology garbled until they seem to say that the structural differences between man and even the highest apes are small and insignificant. Let me take this opportunity then of distinctly asserting, on the contrary, that they are great and significant ; that every bone of a Gorilla bears marks by which it might be distinguished from the corresponding bone of a man ; and that, in the present creation, at any rate, no intermediate link bridges over the gap between *Homo* and *Troglodytes*.

It would be no less wrong than absurd to deny the existence of this chasm ; but it is at least equally wrong and absurd to exaggerate its magnitude, and, resting on the admitted fact of its existence, to refuse to inquire whether it is wide or narrow. Remember, if you will, that there is no existing link between Man and the Gorilla, but do not forget that there is a no less sharp line of demarcation, a no less complete absence of any transitional form, between the Gorilla and the Orang, or the Orang and the Gibbon. I say not less sharp, though it is somewhat narrower. The structural differences between Man and the Man-like apes certainly justify our regarding him as constituting a family apart from them ; though inasmuch as he differs less from them than they do from other families of the same order, there can be no justification for placing him in a distinct order.

And believing, as I do, with Cuvier, that the possession of articulate speech is the grand distinctive character of man (whether it be absolutely peculiar to him or not), I find it very easy to comprehend that some equally inconspicuous structural difference may have been the primary cause of the immeasurable and practically infinite divergence of the Human from the Simian Stirps.

And thus the sagacious foresight of the great lawgiver of systematic zoology, Linnæus, becomes justified, and a century of anatomical research brings us back to his conclusion, that man is a member of the same order (for which the Linnæan term PRIMATES ought to be retained) as the Apes and Lemurs. This order is now divisible into seven families, of about equal systematic value : the first, the ANTHROPINI, contains Man alone ; the second, the CATARHINI, embraces the old world apes ; the third, the PLATYRHINI, all new world apes, except the Marmosets ; the fourth, the ARCTOPITHECINI, contains the Marmosets ; the fifth, the LEMURINI, the Lemurs—from which *Cheiromys* should probably be excluded to form a sixth distinct family, the CHEIROMYINI ; while the seventh, the GALEOPITHECINI, contains only the flying Lemur *Galeopithecus*—a strange form which almost touches on the Bats, as the *Cheiromys* puts on a Rodent clothing, and the Lemurs simulate Insectivora.

Perhaps no order of mammals presents us with so extraordinary a series of gradations as this—leading us insensibly from the crown and summit of the animal creation down to creatures from which there is but a step, as it seems, to the lowest, smallest, and least intelligent of the placental Mammalia. It is as if nature herself had foreseen the arrogance of man, and with Roman severity had provided that his intellect, by its very triumphs, should call into prominence the slaves, admonishing the conqueror that he is but dust.

These are the chief facts, this the immediate conclusion from them to which I adverted in the commencement of this Essay. The facts, I believe, cannot be disputed ; and if so, the conclusion appears to me to be inevitable.

But if Man be separated by no greater structural barrier from the brutes than they are from one another—then it seems to follow that if any process of physical causation can be discovered by which the genera and families of ordinary animals have been produced, that process of causation is amply sufficient to account for the origin of Man. In other words, if it could be shown that the Marmosets, for example, have arisen by gradual modification of the ordinary Platyrhini, or that both Marmosets and Platyrhini are modified ramifications of a primitive stock—then there would be no rational ground for doubting that man might have originated in the one case by the gradual modification of a man-like ape, or in the other case as a ramification of the same primitive stock as those apes.

At the present moment but one such process of physical causation has any evidence in its favor ; or, in other words, there is but one hypothesis regarding the origin of species of animals in general which has any scientific existence—that propounded by Mr. Darwin. For Lamarck, sagacious as many of

his views were, mingled them with so much that was crude, and even absurd, as to neutralize the benefit which his originality might have effected had he been a more sober and cautious thinker; and though I have heard of the announcement of a formula touching "the ordained continuous becoming of organic forms," it is obvious that it is the first duty of a hypothesis to be intelligible, and that a quà-quà-versal proposition of this kind, which may be read backward, or forward, or side-ways, with exactly the same amount of signification, does not really exist, though it may seem to do so.

At the present moment, therefore, the question of the relation of man to the lower animals resolves itself, in the end, into the larger question of the tenability or untenability of Mr. Darwin's views. But here we enter upon difficult ground, and it behooves us to define our exact position with the greatest care.

It cannot be doubted, I think, that Mr. Darwin has satisfactorily proved that what he terms selection, or selective modification, must occur, and does occur, in nature; and he has also proved to superfluity that such selection is competent to produce forms as distinct, structurally, as some genera even are. If the animated world presented us with none but structural differences, I should have no hesitation in saying that Mr. Darwin has demonstrated the existence of a true physical cause, amply competent to account for the origin of living species, and of man among the rest.

But, in addition to their structural distinctions, the species of animals and plants, or at least a great number of them, exhibit physiological characters—what are known as distinct species, structurally, being for the most part either altogether incompetent to breed one with another; or if they breed, the resulting mule, or hybrid, is unable to perpetuate its race with another hybrid of the same kind.

A true physical cause is, however, admitted to be such only on one condition—that it shall account for all the phenomena which come within the range of its operation. If it is inconsistent with any one phenomenon, it must be rejected; if it fails to explain any one phenomenon, it is so far weak, so far to be suspected, though it may have a perfect right to claim provisional acceptance.

Now Mr. Darwin's hypothesis is not, so far as I am aware, inconsistent with any known biological fact; on the contrary, if admitted, the facts of Development, of Comparative Anatomy, of Geographical Distribution, and of Palæontology, become connected together, and exhibit a meaning such as they never possessed before: and I, for one, am fully convinced that, if not precisely true, that hypothesis is as near an approximation to the truth as, for example, the Copernican hypothesis was to the true theory of the planetary motions.

But, for all this, our acceptance of the Darwinian hypothesis must be provisional so long as one link in the chain of evidence is wanting; and so long as all the animals and plants certainly produced by selective breeding from a common stock are fertile, and their progeny are fertile with one another, that link will be wanting. For, so long, selective breeding will not be proved to be competent to do all that is required of it to produce natural species.

I have put this conclusion as strongly as possible before the reader, because the last position in which I wish to find myself is that of an advocate for Mr. Darwin's, or any other views—if by an advocate is meant one whose business it is to smooth over real difficulties, and to persuade where he cannot convince.

In justice to Mr. Darwin, however, it must be admitted that the conditions of fertility and sterility are very ill understood, and that every day's advance in knowledge leads us to regard the hiatus in his evidence as of less and less importance, when set against the multitude of facts which harmonize with, or receive an explanation from, his doctrines.

I adopt Mr. Darwin's hypothesis, therefore, subject to the production of proof that physiological species may be produced by selective breeding; just as a physical philosopher may accept the undulatory theory of light, subject to the proof of the existence of the hypothetical ether; or as the chemist adopts the atomic theory, subject to the proof of the existence of atoms; and for exactly the same reasons, namely, that it has an immense amount of primâ facie probability: that it is the only means at present within reach of reducing the chaos of observed facts to order; and, lastly, that it is the most powerful instrument of investigation which has been presented to naturalists since the invention of the natural system of classification and the commencement of the systematic study of embryology.

But even leaving Mr. Darwin's views aside, the whole analogy of natural operations furnishes so complete and crushing an argument against the intervention of any but what are termed secondary causes, in the production of all the phenomena of the universe, that, in view of the intimate relations between Man and the rest of the living world, and between the forces exerted by the latter and all other forces, I can see no excuse for doubting that all are co-ordinated terms of nature's great progression, from the formless to the formed—from the inorganic to the organic—from blind force to conscious intellect and will.

Science has fulfilled her function when she has ascertained and enunciated truth; and were these pages addressed to men of science only, I should now close this Essay, knowing that my colleagues have learned to respect nothing but evidence, and to believe that their highest duty lies in submitting to it, however it may jar against their inclinations.

But desiring, as I do, to reach the wider circle of the intelligent public, it would be

unworthy cowardice were I to ignore the repugnance with which the majority of my readers are likely to meet the conclusions to which the most careful and conscientious study I have been able to give to this matter has led me.

On all sides I shall hear the cry, "We are men and women, and not a mere better sort of apes, a little longer in the leg, more compact in the foot, and bigger in brain than your brutal Chimpanzees and Gorillas. The power of knowledge—the conscience of good and evil—the pitiful tenderness of human affections, raise us out of all real fellowship with the brutes, however closely they may seem to approximate us."

To this I can only reply that the exclamation would be most just, and would have my own entire sympathy, if it were only relevant. But it is not I who seek to base Man's dignity upon his great toe, or insinuate that we are lost if an Ape has a hippocampus minor. On the contrary, I have done my best to sweep away this vanity. I have endeavored to show that no absolute structural line of demarcation, wider than that between the animals which immediately succeed us in the scale, can be drawn between the animal world and ourselves; and I may add the expression of my belief that the attempt to draw a physical distinction is equally futile, and that even the highest faculties of feeling and of intellect begin to germinate in lower forms of life. At the same time no one is more strongly convinced than I am of the vastness of the gulf between civilized man and the brutes; or is more certain that whether *from* them or not, he is assuredly not *of* them. No one is less disposed to think lightly of the present dignity, or despairingly of the future hopes, of the only consciously intelligent denizen of this world. We are indeed told by those who assume authority in these matters that the two sets of opinions are incompatible, and that the belief in the unity of origin of man and brutes involves the brutalization and degradation of the former. But is this really so? Could not a sensible child confute, by obvious arguments, the shallow rhetoricians who would force this conclusion upon us? Is it indeed true that the Poet, or the Philosopher, or the Artist whose genius is the glory of his age, is degraded from his high estate by the undoubted historical probability, not to say certainty, that he is the direct descendant of some naked and bestial savage, whose intelligence was just sufficient to make him a little more cunning than the Fox, and by so much more dangerous than the Tiger? Or is he bound to howl and grovel on all fours because of the wholly unquestionable fact that he was once an egg, which no ordinary power of discrimination could distinguish from that of a Dog? Or is the philanthropist or the saint to give up his endeavors to lead a noble life because the simplest study of man's nature reveals, at its foundations, all the selfish passions and fierce appetites of the merest quadruped? Is mother-love vile because a hen shows it, or fidelity base because dogs possess it?

The common-sense of the mass of mankind will answer these questions without a moment's hesitation. Healthy humanity, finding itself hard pressed to escape from real sin and degradation, will leave the brooding over speculative pollution to the cynics and the rightous "overmuch," who, disagreeing in everything else, unite in blind insensibility to the nobleness of the visible world, and in inability to appreciate the grandeur of the place Man occupies therein.

Nay, more; thoughtful men, once escaped from the blinding influences of traditional prejudice, will find in the lowly stock whence man has sprung the best evidence of the splendor of his capacities, and will discern in his long progress through the Past a reasonable ground of faith in his attainment of a nobler Future.

They will remember that in comparing civilized man with the animal world one is as the Alpine traveller, who sees the mountains soaring into the sky, and can hardly discern where the deep-shadowed crags and roseate peaks end, and where the clouds of heaven begin. Surely the awe-struck voyager may be excused if at first he refuses to believe the geologist, who tells him that these glorious masses are, after all, the hardened mud of primeval seas, or the cooled slag of subterranean furnaces—of one substance with the dullest clay, but raised by inward forces to that place of proud and seemingly inaccessible glory.

But the geologist is right; and due reflection on his teachings, instead of diminishing our reverence and our wonder, adds all the force of intellectual sublimity to the more æsthetic intuition of the uninstructed beholder.

And after passion and prejudice have died away, the same result will attend the teachings of the naturalist respecting that great Alps and Andes of the living world—Man. Our reverence for the nobility of manhood will not be lessened by the knowledge that Man is, in substance and in structure, one with the brutes; for he alone possesses the marvellous endowment of intelligible and rational speech whereby, in the secular period of his existence, he has slowly accumulated and organized the experience which is almost wholly lost with the cessation of every individual life in other animals; so that now he stands raised upon it as on a mountain-top, far above the level of his humble fellows, and transfigured from his grosser nature by reflecting, here and there, a ray from the infinite source of truth.

A SUCCINCT HISTORY OF THE CONTROVERSY RESPECTING THE CEREBRAL STRUCTURE OF MAN AND THE APES.

Up to the year 1857 all anatomists of authority, who had occupied themselves with the cerebral structure of the Apes—Cuvier, Tiedemann, Sandifort, Vrolik, Isidore G. St. Hilaire, Schroeder van der Kolk, Gratiolet—

were agreed that the brain of the Ape possesses a POSTERIOR LOBE.

Tiedemann, in 1825, figured and acknowledged in the text of his "Icones" the existence of the POSTERIOR CORNU of the lateral ventricle in the Apes, not only under the title of "Scrobiculus parvus loco cornu posterioris"—a fact which has been paraded—but as "cornu posterius" (Icones, p. 54), a circumstance which has been as sedulously kept in the background.

Cuvier (Leçons, T. iii. p. 103) says: "The anterior or lateral ventricles possess a digital cavity [posterior cornu] only in Man and the Apes. . . . Its presence depends on that of the posterior lobes."

Schroeder van der Kolk, and Vrolik, and Gratiolet, had also figured and described the posterior cornu in various Apes. As to the HIPPOCAMPUS MINOR, Tiedemann had erroneously asserted its absence in the Apes; but Schroeder van der Kolk and Vrolik had pointed out the existence of what they considered a rudimentary one in the Chimpanzee, and Gratiolet had expressly affirmed its existence in these animals. Such was the state of our information on these subjects in the year 1856.

In the year 1857, however, Professor Owen, either in ignorance of these well-known facts or else unjustifiably suppressing them, submitted to the Linnæan Society a paper "On the Characters, Principles of Division, and Primary Groups of the Class Mammalia," which was printed in the Society's Journal, and contains the following passage: "In Man the brain presents an ascensive step in development, higher and more strongly marked than that by which the preceding sub class was distinguished from the one below it. Not only do the cerebral hemispheres overlap the olfactory lobes and cerebellum, but they extend in advance of the one and farther back than the other. The posterior development is so marked that anatomists have assigned to that part the character of a third lobe; *it is peculiar to the genus Homo, and equally peculiar is the posterior horn of the lateral ventricle and the "hippocampus minor" which characterize the hind lobe of each hemisphere.*"—*Journal of the Proceedings of the Linnæan Society*, Vol. ii., p. 19.

As the essay in which this passage stands had no less ambitious an aim than the remodelling of the classification of the Mammalia, its author might be supposed to have written under a sense of peculiar responsibility, and to have tested, with especial care, the statements he ventured to promulgate. And even if this be expecting too much, hastiness or want of opportunity for due deliberation cannot now be pleaded in extenuation of any shortcomings; for the propositions cited were repeated two years afterward in the Reade Lecture, delivered before so grave a body as the University of Cambridge, in 1859.

When the assertions which I have italicized in the above extract first came under my notice I was not a little astonished at so flat a contradiction of the doctrines current among well-informed anatomists; but, not unnaturally, imagining that the deliberate statements of a responsible person must have some foundation in fact, I deemed it my duty to investigate the subject anew before the time at which it would be my business to lecture thereupon came round. The result of my inquiries was to prove that Mr. Owen's three assertions—that "the third lobe, the posterior horn of the lateral ventricle, and the hippocampus minor," are "peculiar to the genus *Homo*"—are contrary to the plainest facts. I communicated this conclusion to the students of my class; and then, having no desire to embark in a controversy which could not redound to the honor of British science, whatever its issue, I turned to more congenial occupations.

The time speedily arrived, however, when a persistence in this reticence would have involved me in an unworthy paltering with truth.

At the meeting of the British Association at Oxford, in 1860, Professor Owen repeated these assertions in my presence, and of course I immediately gave them a direct and unqualified contradiction, pledging myself to justify that unusual procedure elsewhere. I redeemed that pledge by publishing, in the January number of the *Natural History Review* for 1861, an article wherein the truth of the three following propositions was fully demonstrated.

"1. That the third lobe is neither peculiar to nor characteristic of man, seeing that it exists in all the higher quadrumana.

"2. That the posterior cornu of the lateral ventricle is neither peculiar to nor characteristic of man, inasmuch as it also exists in the higher quadrumana.

"3. That the *hippocamus minor* is neither peculiar to nor characteristic of man, as it is found in certain of the higher quadrumana."

Furthermore, this paper contains the following paragraph.

"And, lastly, Schroeder van der Kolk and Vrolik (op. cit. p. 271), though they particularly note that 'the lateral ventricle is distinguished from that of Man by the very defective proportions of the posterior cornu, wherein only a stripe is visible as an indication of the hippocampus minor,' yet the Figure 4, in their second plate, shows that this posterior cornu is a perfectly distinct and unmistakable structure, quite as large as it often is in Man. It is the more remarkable that Professor Owen should have overlooked the explicit statement and figure of these authors, as it is quite obvious, on comparison of the figures, that his wood-cut of the brain of a Chimpanzee (l. c. p. 19) is a reduced copy of the second figure of Messrs. Schroeder van der Kolk and Vrolik's first plate.

"As M. Gratiolet (l. c. p. 18), however, is careful to remark, 'unfortunately the brain which they have taken as a model was greatly altered (profondément affaissé),

whence the general form of the brain is given in these plates in a manner which is altogether incorrect.' Indeed, it is perfectly obvious, from a comparison of a section of the skull of the Chimpanzee with these figures, that such is the case; and it is greatly to be regretted that so inadequate a figure should have been taken as a typical representation of the Chimpanzee's brain."

From this time forth the untenability of his position might have been as apparent to Professor Owen as it was to every one else; but, so far from retracting the grave errors into which he had fallen, Professor Owen has persisted in and reiterated them; first, in a lecture delivered before the Royal Institution on the 19th of March, 1861, which is admitted to have been accurately reproduced in the *Athenæum* for the 23d of the same month, in a letter addressed by Professor Owen to that journal on the 30th of March. The *Athenæum* report was accompanied by a diagram purporting to represent a Gorilla's brain, but in reality so extraordinary a misrepresentation that Professor Owen substantially, though not explicitly, withdraws it in the letter in question. In amending this error, however, Professor Owen fell into another of much graver import, as his communication concludes with the following paragraph: "For the true proportion in which the cerebrum covers the cerebellum in the highest Apes, reference should be made to the figure of the undissected brain of the Chimpanzee in my 'Reade's Lecture on the Classification, etc., of the Mammalia,' p. 25, Fig. 7, 8vo. 1859."

It would not be credible, if it were not unfortunately true, that this figure, to which the trusting public is referred, without a word of qualification, "for the true proportion in which the cerebrum covers the cerebellum in the highest Apes," is exactly that unacknowledged copy of Schroeder van der Kolk and Vrolik's figure whose utter inaccuracy had been pointed out years before by Gratiolet, and had been brought to Professor Owen's knowledge by myself in the passage of my article in the *Natural History Review* above quoted.

I drew public attention to this circumstance again in my reply to Professor Owen, published in the *Athenæum* for April 13th, 1861; but the exploded figure was reproduced once more by Professor Owen, without the slightest allusion to its inaccuracy, in the *Annals of Natural History* for June, 1861!

This proved too much for the patience of the original authors of the figure, Messrs. Schroeder van der Kolk and Vrolik, who, in a note addressed to the Academy of Amsterdam, of which they were members, declared themselves to be, though decided opponents of all forms of the doctrine of progressive development, above all things lovers of truth: and that, therefore, at whatever risk of seeming to lend support to views which they disliked, they felt it their duty to take the first opportunity of publicly repudiating Professor Owen's misuse of their authority.

In this note they frankly admitted the justice of the criticisms of M. Gratiolet, quoted above, and they illustrated, by new and careful figures, the posterior lobe, the posterior cornu, and the hippocampus minor of the Orang. Furthermore, having demonstrated the parts, at one of the sittings of the Academy, they add, "la présence des parties contestées y a été universellement reconnue par les anatomistes présents à la séance. Le seul doute qui soit resté se rapporte au pes Hippocampi minor. . . . A l'état frais l'indice du petit pied d'Hippocampe était plus prononcé que maintenant."

Professor Owen repeated his erroneous assertions at the meeting of the British Association in 1861, and again, without any obvious necessity, and without adducing a single new fact or new argument, or being able in any way to meet the crushing evidence from original dissections of numerous Apes' brains, which had in the mean while been brought forward by Professor Rolleston,* F.R.S., Mr. Marshall, † F.R.S., Mr. Flower, ‡ Mr. Turner, § and myself, ‖ revived the subject at the Cambridge meeting of the same body in 1862. Not content with the tolerably vigorous repudiation which these unprecedented proceedings met with in Section D, Professor Owen sanctioned the publication of a version of his own statements, accompanied by a strange misrepresentation of mine (as may be seen by comparison of the *Times'* report of the discussion), in the *Medical Times* for October 11th, 1862. I subjoin the conclusion of my reply in the same journal for October 25th:

"If this were a question of opinion, or a question of interpretation of parts or of terms—were it even a question of observation, in which the testimony of my own senses alone was pitted against that of another person, I should adopt a very different tone in discussing this matter. I should, in all humility, admit the likelihood of having myself erred in judgment, failed in knowledge, or been blinded by prejudice.

"But no one pretends now that the controversy is one of terms or of opinions. Novel and devoid of authority as some of Professor Owen's proposed definitions may have been, they might be accepted without changing the great features of the case. Hence, though special investigations into these matters have been undertaken during the last two years by Dr. Allen Thomson, by Dr. Rolleston, by Mr. Marshall, and by Mr. Flower, all, as you are aware, anatomists of repute in this country, and by Professors Schroeder van der Kolk and Vrolik (whom Professor Owen

* On the Affinities of the Brain of the Orang. Nat. Hist. Review, April, 1861.

† On the Brain of a young Chimpanzee. Ibid. July, 1861.

‡ On the Posterior Lobes of the Cerebrum of the Quadrumana. Philosophical Transactions, 1862.

§ On the Anatomical Relations of the Surfaces of the Tentorium to the Cerebrum and Cerebellum in Man and the Lower Mammals. Proceedings of the Royal Society of Edinburgh, March, 1862.

‖ On the Brain of Ateles. Proceedings of Zoological Society, 1861.

incautiously tried to press into his own service) on the Continent, all these able and conscientious observers have with one accord testified to the accuracy of my statements, and to the utter baselessness of the assertions of Professor Owen. Even the venerable Rudolph Wagner, whom no man will accuse of progressional proclivities, has raised his voice on the same side; while not a single anatomist, great or small, has supported Professor Owen.

"Now I do not mean to suggest that scientific differences should be settled by universal suffrage, but I do conceive that solid proofs must be met by something more than empty and unsupported assertions. Yet during the two years through which this preposterous controversy has dragged its weary length, Professor Owen has not ventured to bring forward a single preparation in support of his often-repeated assertions.

"The case stands thus, therefore: Not only are the statements made by me in consonance with the doctrines of the best older authorities, and with those of all recent investigators, but I am quite ready to demonstrate them on the first monkey that comes to hand; while Professor Owen's assertions are not only in diametrical opposition to both old and new authorities, but he has not produced, and, I will add, cannot produce, a single preparation which justifies them."

I now leave this subject for the present. For the credit of my calling I should be glad to be, hereafter, forever silent upon it. But, unfortunately, this is a matter upon which, after all that has occurred, no mistake or confusion of terms is possible—and in affirming that the posterior lobe, the posterior cornu, and the hippocampus minor exist in certain Apes, I am stating either that which is true or that which I must know to be false. The question has thus become one of personal veracity. For myself, I will accept no other issue than this, grave as it is, to the present controversy.

III.

ON SOME FOSSIL REMAINS OF MAN.

I HAVE endeavored to show, in the preceding essay, that the ANTHROPINI, or Man Family, form a very well defined group of the Primates, between which and the immediately following Family, the CATARHINI, there is, in the existing world, the same entire absence of any transitional form or connecting link, as between the CATARHINI and PLATYRHINI.

It is a commonly received doctrine, however, that the structural intervals between the various existing modifications of organic beings may be diminished or even obliterated, if we take into account the long and varied succession of animals and plants which have preceded those now living, and which are known to us only by their fossilized remains. How far this doctrine is well based, how far, on the other hand, as our knowledge at present stands, it is an overstatement of the real facts of the case, and an exaggeration of the conclusions fairly deducible from them, are points of grave importance, but into the discussion of which I do not, at present, propose to enter. It is enough that such a view of the relations of extinct to living beings has been propounded, to lead us to inquire, with anxiety, how far the recent discoveries of human remains in a fossil state bear out, or oppose, that view.

I shall confine myself, in discussing this question, to those fragmentary Human skulls from the caves of Engis in the valley of the Meuse, in Belgium, and of the Neanderthal near Düsseldorf, the geological relations of which have been examined with so much care by Sir Charles Lyell upon whose high authority I shall take it for granted that the Engis skull belonged to a contemporary of the Mammoth (*Elephas primigenius*) and of the woolly Rhinoceros (*Rhinocerus tichorhinus*), with the bones of which it was found associated; and that the Neanderthal skull is of great, though uncertain, antiquity. Whatever be the geological age of the latter skull, I conceive it is quite safe (on the ordinary principles of paleontological reasoning) to assume that the former takes us to, at least, the farther side of the vague biological limit which separates the present geological epoch from that which immediately preceded it. And there can be no doubt that the physical geography of Europe has changed wonderfully since the bones of Men and Mammoths, Hyenas and Rhinoceroses were washed pell-mell into the cave of Engis.

The skull from the cave of Engis was originally discovered by Professor Schmerling, and was described by him, together with other human remains disinterred at the same time, in his valuable work, "Recherches sur les ossemens fossiles découverts dans les cavernes de la Province de Liége," published in 1833 (p. 59, *et seq.*), from which the following paragraphs are extracted, the precise expressions of the author being, as far as possible, preserved:

"In the first place I must remark that these human remains, which are in my possession, are characterized, like the thousands of bones which I have lately been disinterring, by the extent of the decomposition which they have undergone, which is precisely the same as that of the extinct species: all, with a few exceptions, are broken; some few are rounded, as is frequently found to be the case in fossil remains of other species. The fractures are vertical or oblique; none of them are eroded; their color does not differ from that of other fossil bones, and varies from whitish yellow to blackish. All are lighter than recent bones, with the exception of those which have a calcareous incrustation, and the cavities of which are filled with such matter.

"The cranium which I have caused to be figured, Plate I. Figs. 1, 2, is that of an old person. The sutures are beginning to be effaced: all the facial bones are wanting,

and of the temporal bones only a fragment of that of the right side is preserved.

"The face and the base of the cranium had been detached before the skull was deposited in the cave, for we were unable to find those parts though the whole cavern was regularly searched. The cranium was met with at a depth of a metre and a half [five feet nearly], hidden under an osseous breccia, composed of the remains of small animals, and containing one rhinoceros tusk, with several teeth of horses and of ruminants. This breccia, which has been spoken of above (p. 31), was a metre [3½ feet about] wide, and rose to the height of a metre and a half above the floor of the cavern, to the walls of which it adhered strongly.

"The earth which contained this human skull exhibited no trace of disturbance: teeth of rhinoceros, horse, hyena, and bear, surrounded it on all sides.

"The famous Blumenbach* has directed attention to the differences presented by the form and the dimensions of human crania of different races. This important work would have assisted us greatly if the face, a part essential for the determination of race, with more or less accuracy, had not been wanting in our fossil cranium.

"We are convinced that even if the skull had been complete, it would not have been possible to pronounce, with certainty, upon a single specimen; for individual variations are so numerous in the crania of one and the same race that one cannot, without laying one's self open to large chances of error, draw any inference from a single fragment of a cranium to the general form of the head to which it belonged.

"Nevertheless, in order to neglect no point respecting the form of this fossil skull, we may observe that, from the first, the elongated and narrow form of the forehead attracted our attention.

"In fact, the slight elevation of the frontal, its narrowness, and the form of the orbit, approximate it more nearly to the cranium of an Ethiopian than to that of an European: the elongated form and the produced occiput are also characters which we believe to be observable in our fossil cranium; but to remove all doubt upon that subject I have caused the contours of the cranium of an European and of an Ethiopian to be drawn and the foreheads represented. Plate II., Figs. 1 and 2, and, in the same plate, Figs. 3 and 4, will render the differences easily distinguishable; and a single glance at the figures will be more instructive than a long and wearisome description.

"At whatever conclusion we may arrive as to the origin of the man from whence this fossil skull proceeded, we may express an opinion without exposing ourselves to a fruitless controversy. Each may adopt the hypothesis which seems to him most probable:

* Decas Collectionis suæ cranorium diversarum gentium illustrata. Gottingæ, 1790–1820.

Fig. 23.—The skull from the cave of Engis—viewed from the right side. One half the size of nature. a, glabella; b, occipital protuberance (a to b glabello-occipital line); c, auditory foramen.

for my own part, I hold it to be demonstrated that this cranium has belonged to a person of limited intellectual faculties, and we conclude thence that it belonged to a man of a low degree of civilization: a deduction which is borne out by contrasting the capacity of the frontal with that of the occipital region.

"Another cranium of a young individual was discovered in the floor of the cavern beside the tooth of an elephant; the skull was entire when found, but the moment it was lifted it fell into pieces, which I have not, as yet, been able to put together again. But I have represented the bones of the upper jaw, Plate I., Fig. 5. The state of the alveoli and the teeth shows that the molars had not yet pierced the gum. Detached milk molars and some fragments of a human skull proceed from this same place. The Figure 3 represents a human superior incisor tooth, the size of which is truly remarkable.

"Figure 4 is a fragment of a superior maxillary bone, the molar teeth of which are worn down to the roots.

"I possess two vertebræ, a first and last dorsal.

"A clavicle of the left side (see Plate III., Fig. 1); although it belonged to a young individual, this bone shows that he must have been of great stature.

"Two fragments of the radius, badly preserved, do not indicate that the height of the man, to whom they belonged, exceeded five feet and a half.

"As to the remains of the upper extremities, those which are in my possession consist merely of a fragment of an ulna and of a radius (Plate III., Figs. 5 and 6).

"Figure 2, Plate IV., represents a metacarpal bone, contained in the breccia, of which we have spoken; it was found in the lower part above the cranium: add to this some metacarpal bones found at very different distances, half a dozen metatarsals, three phalanges of the hand and one of the foot.

"This is a brief enumeration of the remains of human bones collected in the cavern of Engis, which has preserved for us the remains of three individuals surrounded by those of the Elephant, of the Rhinoceros, and of Carnivora of species unknown in the present creation."

From the cave of Engihoul, opposite that of Engis, on the right bank of the Meuse, Schmerling obtained the remains of three other individuals of Man, among which were only two fragments of parietal bones, but many bones of the extremities. In one case a broken fragment of an ulna was soldered to a like fragment of a radius by stalagmite, a condition frequently observed among the bones of the Cave Bear (*Ursus spelæus*), found in the Belgian caverns.

It was in the cavern of Engis that Professor Schmerling found, incrusted with stalagmite and joined to a stone, the pointed bone implement, which he has figured in Fig. 7 of his Plate XXXVI., and worked flints were found by him in all those Belgian caves, which contained an abundance of fossil bones.

A short letter from M. Geoffroy St. Hilaire, published in the "Comptes Rendus of the Academy of Sciences of Paris" for July 2d, 1838, speaks of a visit (and apparently a very hasty one) paid to the collection of Professor "Schermidt" (which is presumably a misprint for Schmerling) at Liège. The writer briefly criticises the drawings which illustrate Schmerling's work, and affirms that the "human cranium is a little longer than it is represented" in Schmerling's figure. The only other remark worth quoting is this: "The aspect of the human bones differs little from that of the cave bones, with which we are familiar, and of which there is a considerable collection in the same place. With respect to their special forms, compared with those of the varieties of recent human crania, few *certain* conclusions can be put forward; for much greater differences exist between the different specimens of well-characterized varieties than between the fossil cranium of Liège, and that of one of those varieties selected as a term of comparison."

Geoffroy St. Hilaire's remarks are, it will be observed, little but an echo of the philosophic doubts of the describer and discoverer of the remains. As to the critique upon Schmerling's figures, I find that the side view given by the latter is really about $\frac{3}{10}$ths of an inch shorter than the original, and that the front view is diminished to about the same extent. Otherwise the representation is not, in any way, inaccurate, but corresponds very well with the cast which is in my possession.

A piece of the occipital bone, which Schmerling seems to have missed, has since been fitted on to the rest of the cranium by an accomplished anatomist, Dr. Spring of Liège, under whose direction an excellent plaster cast was made for Sir Charles Lyell. It is upon and from a duplicate of that cast that my own observations and the accompanying figures, the outlines of which are copied from the very accurate camera lucida drawings by my friend Mr. Busk, reduced to one half of the natural size, are made.

As Professor Schmerling observes, the base of the skull is destroyed, and the facial bones are entirely absent; but the roof of the cranium, consisting of the frontal, parietal, and the greater part of the occipital bones, as far as the middle of the occipital foramen, is entire, or nearly so. The left temporal bone is wanting. Of the right temporal, the parts in the immediate neighborhood of the auditory foramen, the mastoid process, and a considerable portion of the squamous element of the temporal, are well preserved (Fig. 22).

The lines of fracture which remain between the coadjusted pieces of the skull, and are faithfully displayed in Schmerling's figure, are readily traceable in the cast. The sutures are also discernible, but the complex disposition of their serrations, shown in the figure, is not obvious in the cast. Though

the ridges which give attachment to muscles are not excessively prominent, they are well marked, and taken together with the apparently well developed frontal sinuses, and the condition of the sutures, leave no doubt on my mind that the skull is that of an adult if not middle-aged man.

The extreme length of the skull is 7.7 inches. Its extreme breadth, which corresponds very nearly with the interval between the parietal protuberances, is not more than 5.4 inches. The proportion of the length to the breadth is therefore very nearly as 100 to 70. If a line be drawn from the point at which the brow curves in toward the root of the nose, and which is called the "glabella" (*a*), (Fig. 22), to the occipital protuberance (*b*), and the distance to the highest point of the arch of the skull be measured perpendicularly from this line, it will be found to be 4.75 inches. Viewed from above, Fig. 23 A, the forehead presents an evenly [illegible] curve, and passes into the [illegible] of the [illegible] back of the skull, which describes a tolerably regular elliptical curve.

FIG. 23.—The Engis skull viewed from above (*A*) and in front (*B*).

The front view (Fig. 23 B) shows that the roof of the skull was very regularly and elegantly arched in the transverse direction, and that the transverse diameter was a little less below the parietal protuberances than above them. The forehead cannot be called narrow in relation to the rest of the skull, nor can it be called a retreating forehead; on the contrary, the antero-posterior contour of the skull is well arched, so that the distance along that contour, from the nasal depression to the occipital protuberance, measures about 13.75 inches. The transverse arc of the skull, measured from one auditory foramen to the other, across the middle of the sagittal suture, is about 13 inches. The sagittal suture itself is 5.5 inches long.

The supraciliary prominences or brow-ridges (on each side of *a*, Fig. 22) are well, but not excessively, developed, and are separated by a median depression. Their principal elevation is disposed so obliquely that I judge them to be due to large frontal sinuses.

If a line joining the glabella and the occipital protuberance (*a*, *b*, Fig. 22) be made horizontal, no part of the occipital region projects more than $\frac{1}{10}$th of an inch behind the posterior extremity of that line, and the upper edge of the auditory foramen (*c*) is almost in contact with a line drawn parallel with this upon the outer surface of the skull.

A transverse line drawn from one auditory foramen to the other traverses, as usual, the forepart of the occipital foramen. The capacity of the interior of this fragmentary skull has not been ascertained.

The history of the Human remains from the cavern in the Neanderthal may best be given in the words of their original describer, Dr. Schaaffhausen, as translated by Mr. Busk:

"In the early part of the year 1857 a human skeleton was discovered in a limestone cave in the Neanderthal, near Hochdal, between Düsseldorf and Elberfeld. Of this, however, I was unable to procure more than a plaster cast of the cranium, taken at Elberfeld, from which I drew up an account of its remarkable conformation, which was, in the first instance, read on the 4th of February, 1857, at the meeting of the Lower Rhine Medical and Natural History Society, at Bonn. Subsequently Dr. Fuhlrott, to whom science is indebted for the preservation of these bones, which were not at first regarded as human, and into whose possession they afterward came, brought the cranium from Elberfeld to Bonn, and intrusted it to me for a more accurate anatomical examination. At the General Meeting of the Natural History Society of Prussian Rhineland and Westphalia at Bonn, on the 2d of June, 1857, Dr. Fuhlrott himself gave a full account of the locality and of the circumstances under which the discovery was made. He was of opinion that the bones might be regarded as fossil; and in coming to this con-

clusion he laid especial stress upon the existence of dendritic deposits, with which their surface was covered, and which were first noticed upon them by Professor Mayer. To this communication I appended a brief report on the results of my anatomical examination of the bones. The conclusions at which I arrived were: 1st. That the extraordinary form of the skull was due to a natural conformation hitherto not known to exist, even in the most barbarous races. 2d. That these remarkable human remains belonged to a period antecedent to the time of the Celts and Germans, and were in all probability derived from one of the wild races of North-western Europe, spoken of by Latin writers; and which were encountered as autochthones by the German immigrants. And 3dly. That it was beyond doubt that these human relics were traceable to a period at which the latest animals of the diluvium still existed; but that no proof of this assumption, nor consequently of their so-termed *fossil* condition, was afforded by the circumstances under which the bones were discovered.

"As Dr. Fuhlrott has not yet published his description of these circumstances, I borrow the following account of them from one of his letters: 'A small cave or grotto, high enough to admit a man, and about 15 feet deep from the entrance, which is 7 or 8 feet wide, exists in the southern wall of the gorge of the Neanderthal, as it is termed, at a distance of about 10 feet from the Düssel, and about 60 feet above the bottom of the valley. In its earlier and uninjured condition, this cavern opened upon a narrow plateau lying in front of it, and from which the rocky wall descended almost perpendicularly into the river. It could be reached, though with difficulty, from above. The uneven floor was covered to a thickness of 4 or 5 feet with a deposit of mud, sparingly intermixed with rounded fragments of chert. In the removing of this deposit the bones were discovered. The skull was first noticed, placed nearest to the entrance of the cavern; and farther in, the other bones, lying in the same horizontal plane. Of this I was assured in the most positive terms by two laborers who were employed to clear out the grotto, and who were questioned by me on the spot. At first no idea was entertained of the bones being human; and it was not till several weeks after their discovery that they were recognized as such by me, and placed in security. But as the importance of the discovery was not at the time perceived, the laborers were very careless in the collecting, and secured chiefly only the larger bones; and to this circumstance it may be attributed that fragments merely of the probably perfect skeleton came into my possession.'

"My anatomical examination of these bones afforded the following results:

"The cranium is of unusual size, and of a long elliptical form. A most remarkable peculiarity is at once obvious in the extraordinary development of the frontal sinuses, owing to which the superciliary ridges, which coalesce completely in the middle, are rendered so prominent that the frontal bone exhibits a considerable hollow or depression above or rather behind them, while a deep depression is also formed in the situation of the root of the nose. The forehead is narrow and low, though the middle and hinder portions of the cranial arch are well developed. Unfortunately, the fragment of the skull that has been preserved consists only of the portion situated above the roof of the orbits and the superior occipital ridges, which are greatly developed, and almost conjoined so as to form a horizontal eminence. It includes almost the whole of the frontal bone, both parietals, a small part of the squamous and the upper third of the occipital. The recently fractured surfaces show that the skull was broken at the time of its disinterment. The cavity holds 16,876 grains of water, whence its cubical contents may be estimated at 57.64 inches, or 1033.24 cubic centimetres. In making this estimation the water is supposed to stand on a level with the orbital plate of the frontal, with the deepest notch in the squamous margin of the parietal, and with the superior semicircular ridges of the occipital. Estimated in dried millet-seed, the contents equalled 31 ounces, Prussian apothecaries' weight. The semicircular line indicating the upper boundary of the attachment of the temporal muscle, though not very strongly marked, ascends nevertheless to more than half the height of the parietal bone. On the right superciliary ridge is observable an oblique furrow or depression, indicative of an injury received during life.* The coronal and sagittal sutures are on the exterior nearly closed, and on the inside so completely ossified as to have left no traces whatever, while the lambdoidal remains quite open. The depressions for the Pacchionian glands are deep and numerous; and there is an unusually deep vascular groove immediately behind the coronal suture, which, as it terminates in a foramen, no doubt transmitted a *vena emissaria*. The course of the frontal suture is indicated externally by a slight ridge; and where it joins the coronal this ridge rises into a small protuberance. The course of the sagittal suture is grooved, and above the angle of the occipital bone the parietals are depressed.

	mm†
The length of the skull from the nasal process of the frontal over the vertex to the superior semicircular lines of the occipital measures	303 (300)=12·0″
Circumference over the orbital ridges and the superior semicircular lines of the occipital	590 (590)=23·37″ or 23″
Width of the frontal from the middle of the temporal line	

* This, Mr. Busk has pointed out, is probably the notch for the frontal nerve.

† The numbers in brackets are those which I should assign to the different measures, as taken from the plaster cast.—G. B.

on one side to the same point on the opposite	104	(114)=4·1″—4·5″
Length of the frontal from the nasal process to the coronal suture	133	(125)=5·25″—5″
Extreme width of the frontal sinuses	25	(23)=1·0″—0·9″
Vertical height above a line joining the deepest notches in the squamous border of the parietals	70	=2·75″
Width of hinder part of skull from one parietal protuberance to the other	138	(150)=5·4″—5·9″
Distance from the upper angle of the occipital to the superior semicircular lines	51	(60)=1·9″—2·4″
Thickness of the bone at the parietal protuberance	8	
Thickness at the angle of the occipital	9	
Thickness at the superior semicircular line of the occipital	10	=0·3″

"Besides the cranium, the following bones have been secured:

"1. Both thigh-bones, perfect. These, like the skull and all the other bones, are characterized by their unusual thickness, and the great development of all the elevations and depressions for the attachment of muscles. In the Anatomical Museum at Bonn, under the designation of 'Giant's bones,' are some recent thigh-bones, with which in thickness the foregoing pretty nearly correspond, although they are shorter.

	GIANT'S BONES. mm.	FOSSIL BONES. mm.
Length	542=21·4″	438=17·4″
Diameter of head of femur	54= 2·14″	53= 2·0″
Diameter of lower articular end, from one condyle to the other	89= 3·5″	87=3·4″
Diameter of femur in the middle	33= 1·2″	30=1·1″

"2. A perfect right humerus, whose size shows that it belongs to the thigh-bones.

	mm.
Length	312=12·3″
Thickness in the middle	26= 1·0″
Diameter of head	49= 1·9″

"Also a perfect right radius of corresponding dimensions, and the upper third of a right ulna corresponding to the humerus and radius.

"3. A left humerus, of which the upper third is wanting, and which is so much slenderer than the right as apparently to belong to a distinct individual; a left *ulna*, which, though complete, is pathologically deformed, the coronoid process being so much enlarged by bony growth that flexure of the elbow beyond a right angle must have been impossible; the anterior fossa of the humerus for the reception of the coronoid process being also filled up with a similar bony growth. At the same time the olecranon is curved strongly downward. As the bone presents no sign of rachitic degeneration, it may be supposed that an injury sustained during life was the cause of the anchylosis. When the left ulna is compared with the right radius, it might at first sight be concluded that the bones respectively belonged to different individuals, the ulna being more than half an inch too short for articulation with a corresponding radius. But it is clear that this shortening, as well as the attenuation of the left humerus, are both consequent upon the pathological condition above described.

"4. A left *ilium*, almost perfect, and belonging to the femur; a fragment of the right *scapula*; the anterior extremity of a rib of the right side; and the same part of a rib of the left side; the hinder part of a rib of the right side; and, lastly, two hinder portions and one middle portion of ribs, which, from their unusually rounded shape and abrupt curvature, more resemble the ribs of a carnivorous animal than those of a man. Dr. H. v. Meyer, however, to whose judgment I defer, will not venture to declare them to be ribs of any animal; and it only remains to suppose that this abnormal condition has arisen from an unusually powerful development of the thoracic muscles.

"The bones adhere strongly to the tongue, although, as proved by the use of hydrochloric acid, the greater part of the cartilage is still retained in them, which appears, however, to have undergone that transformation into gelatine which has been observed by v. Bibra in fossil bones. The surface of all the bones is in many spots covered with minute black specks, which, more especially under a lens, are seen to be formed of very delicate *dendrites*. These deposits, which were first observed on the bones by Dr. Mayer, are most distinct on the inner surface of the cranial bones. They consist of a ferruginous compound, and, from their black color, may be supposed to contain manganese. Similar dendritic formations also occur, not unfrequently, on laminated rocks, and are usually found in minute fissures and cracks. At the meeting of the Lower Rhine Society at Bonn, on the 1st of April, 1857, Professor Mayer stated that he had noticed in the museum of Poppelsdorf similar dendritic crystallizations on several fossil bones of animals, and particularly on those of *Ursus spelæus*, but still more abundantly and beautifully displayed on the fossil bones and teeth of *Equus adamiticus*, *Elephas primigenius*, etc., from the caves of Bolve and Sundwig. Faint indications of similar *dendrites* were visible in a Roman skull from Siegburg; while other ancient skulls, which had lain for centuries in the earth, presented no trace of them. I am indebted to H. v. Meyer for the following remarks on this subject:

"'The incipient formation of dendritic deposits, which were formerly regarded as a sign of a truly fossil condition, is interesting. It has even been supposed that in diluvial deposits the presence of *dendrites* might be regarded as affording a certain mark of distinction between bones mixed with the diluvium at a somewhat later period and the true diluvial relics, to which alone it was supposed that these deposits were confined. But I have long been convinced that neither can the absence of *dendrites* be regarded as indicative of recent age, nor their presence as sufficient to establish the great an-

tiquity of the objects upon which they occur. I have myself noticed upon paper, which could scarcely be more than a year old, dendritic deposits, which could not be distinguished from those on fossil bones. Thus I possess a dog's skull from the Roman colony of the neighboring Heddersheim, *Castrum Hadrianum*, which is in no way distinguishable from the fossil bones from the Frankish caves; it presents the same color, and adheres to the tongue just as they do; so that this character also, which, at a former meeting of German naturalists at Bonn, gave rise to amusing scenes between Buckland and Schmerling, is no longer of any value. In disputed cases, therefore, the condition of the bone can scarcely afford the means for determining with certainty whether it be fossil, that is to say, whether it belong to geological antiquity or to the historical period.'

"As we cannot now look upon the primitive world as representing a wholly different condition of things, from which no transition exists to the organic life of the present time, the designation of *fossil* as applied to *a bone* has no longer the sense it conveyed in the time of Cuvier. Sufficient grounds exist for the assumption that man coexisted with the animals found in the *diluvium;* and many a barbarous race may, before all historical time, have disappeared together with the animals of the ancient world, while the races whose organization is improved have continued the genus The bones which form the subject of this paper present characters which, although not decisive as regards a geological epoch, are, nevertheless, such as indicate a very high antiquity. It may also be remarked that, common as is the occurrence of diluvial animal bones in the muddy deposits of caverns, such remains have not hitherto been met with in the caves of the Neanderthal; and that the bones, which were covered by a deposit of mud not more than four or five feet thick, and without any protective covering of stalagmite, have retained the greatest part of their organic substance.

"These circumstances might be adduced against the probability of a geological antiquity. Nor should we be justified in regarding the cranial conformation as perhaps representing the most savage primitive type of the human race, since crania exist among living savages, which, though not exhibiting such a remarkable conformation of the forehead, which gives the skull somewhat the aspect of that of the large apes, still in other respects, as for instance in the greater depth of the temporal fossæ, the crest-like, prominent temporal ridges, and a generally less capacious cranial cavity, exhibit an equally low stage of development. There is no reason for supposing that the deep frontal hollow is due to any artificial flattening, such as is practised in various modes by barbarous nations in the Old and New World. The skull is quite symmetrical, and shows no indication of counter pressure at the occiput, while, according to Morton, in the Flatheads of the Columbia, the frontal and parietal bones are always unsymmetrical. Its conformation exhibits the sparing development of the anterior part of the head which has been so often observed in very ancient crania, and affords one of the most striking proofs of the influence of culture and civilization on the form of the human skull."

In a subsequent passage, Dr. Shaaffhausen remarks:

"There is no reason whatever for regarding the unusual development of the frontal sinuses in the remarkable skull from the Neanderthal as an individual or pathological deformity: it is unquestionably a typical race-character, and is physiologically connected with the uncommon thickness of the other bones of the skeleton, which exceeds by about one half the usual proportions. This expansion of the frontal sinuses, which are appendages of the air-passages, also indicates an unusual force and power of endurance in the movements of the body, as may be concluded from the size of all the ridges and processes for the attachment of the muscles or bones. That this conclusion may be drawn from the existence of large frontal sinuses, and a prominence of the lower frontal region, is confirmed in many ways by other observations. By the same characters, according to Pallas, the wild horse is distinguished from the domesticated, and, according to Cuvier, the fossil cave-bear from every recent species of bear, while, according to Roulin, the pig, which has become wild in America, and regained a resemblance to the wild boar, is thus distinguished from the same animal in the domesticated state, as is the chamois from the goat; and, lastly, the bull-dog, which is characterized by its large bones and strongly-developed muscles from every other kind of dog. The estimation of the facial angle, the determination of which, according to Professor Owen, is also difficult in the great apes, owing to the very prominent supra orbital ridges, in the present case is rendered still more difficult from the absence both of the auditory opening and of the nasal spine. But if the proper horizontal position of the skull be taken from the remaining portions of the orbital plates, and the ascending line made to touch the surface of the frontal bone behind the prominent supra-orbital ridges, the facial angle is not found to exceed 56°. Unfortunately, no portions of the facial bones, whose conformation is so decisive as regards the form and expression of the head, have been preserved. The cranial capacity, compared with the uncommon strength of the corporeal frame, would seem to indicate a small cerebral development. The skull, as it is, holds about 31 ounces of millet-seed; and as, from the proportionate size of the wanting bones, the whole cranial cavity should have about six ounces more added, the contents, were it perfect, may be taken at 37 ounces. Tiedemann assigns, as the cranial contents in the Negro, 40, 38, and 35 ounces. The cranium holds

FIG. 24.—The skull from the Neanderthal cavern. *A*, side, *B*, front, and *C*, top view. One half the natural size. The outlines from camera lucida drawings, one half the natural size, by Mr. Busk; the details from the cast and from Dr. Fuhlrott's photographs. *a*, glabella; *b*, occipital protuberance; *d*, lambdoidal suture.

rather more than 36 ounces of water, which corresponds to a capacity of 1033.24 cubic centimetres. Huschke estimates the cranial contents of a Negress at 1127 cubic centimetres; of an old Negro at 1146 cubic centimetres. The capacity of the Malay skulls, estimated by water, equalled 36, 33 ounces, while in the diminutive Hindoos it falls to as little as 27 ounces."

After comparing the Neanderthal cranium with many others, ancient and modern, Professor Schaaffhausen concludes thus:

"But the human bones and cranium from the Neanderthal exceed all the rest in those peculiarities of conformation which lead to the conclusion of their belonging to a barbarous and savage race. Whether the cavern in which they were found, unaccompanied with any trace of human art, were the place of their interment, or whether, like the bones of extinct animals elsewhere, they had been washed into it, they may still be regarded as the most ancient memorial of the early inhabitants of Europe."

Mr. Busk, the translator of Dr. Schaaffhausen's paper, has enabled us to form a very vivid conception of the degraded character of the Neanderthal skull, by placing side by side with its outline that of the skull of a Chimpanzee, drawn to the same absolute size.

Some time after the publication of the translation of Professor Schaaffhausen's "Memoir," I was led to study the cast of the Neanderthal cranium with more attention than I had previously bestowed upon it, in consequence of wishing to supply Sir Charles Lyell with a diagram, exhibiting the special peculiarities of this skull, as compared with other human skulls. In order to do this it was necessary to identify, with precision, those points in the skulls compared which corresponded anatomically. Of these points, the glabella was obvious enough; but when I had distinguished another, defined by the occipital protuberance and superior semicircular line, and had placed the outline of the Neanderthal skull against that of the Engis skull in such a position that the glabella and occipital protuberance of both were intersected by the same straight line, the difference was so vast and the flattening of the Neanderthal skull so prodigious (compare Figs. 22 and 24 A), that I at first imagined I must have fallen into some error. And I was the more inclined to suspect this, as, in ordinary human skulls, the occipital protuberance and superior semicircular curved line on the exterior of the occiput correspond pretty closely with the "lateral sinuses" and the line of attachment of the tentorium internally. But on the tentorium rests, as I have said in the preceding essay, the posterior lobe of the brain; and hence the occipital protuberance and the curved line in question indicate, approximately, the lower limits of that lobe. Was it possible for a human being to have the brain thus flattened and depressed; or, on the other hand, had the muscular ridges shifted their position? In order to solve these doubts, and to decide the question whether the great supraciliary projections did, or did not, arise from the development of the frontal sinuses, I requested Sir Charles Lyell to be so good as to obtain for me from Dr. Fuhlrott, the possessor of the skull, answers to certain queries, and if possible a cast, or at any rate drawings, or photographs, of the interior of the skull.

Dr. Fuhlrott replied, with a courtesy and readiness for which I am infinitely indebted to him, to my inquiries, and furthermore sent three excellent photographs. One of these gives a side view of the skull, and from it Fig. 24 A has been shaded. The second (Fig. 25 A) exhibits the wide openings of the

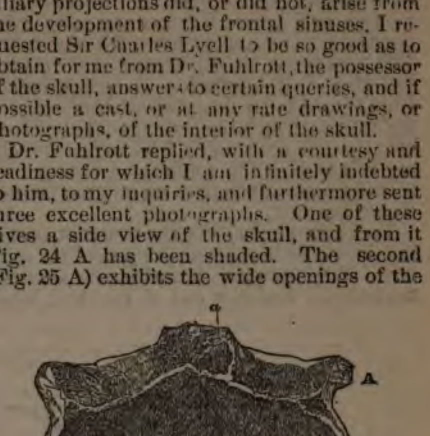


FIG. 25.—Drawings from Dr. Fuhlrott's photographs of parts of the interior of the Neanderthal cranium. *A*, view of the under and inner surface of the frontal region, showing the inferior apertures of the frontal sinuses (*a*). *B*, corresponding view of the occipital region of the skull, showing the impressions of the lateral sinuses (*aa*).

frontal sinuses upon the inferior surface of the frontal part of the skull, into which, Dr. Fuhlrott writes, "a probe may be introduced to the depth of an inch," and demonstrates the great extension of the thickened supraciliary ridges beyond the cerebral cavity. The third, lastly (Fig. 25 B), exhibits the edge and the interior of the posterior, or occipital, part of the skull, and shows very clearly the two depressions for the lateral sinuses, sweeping inward toward the middle line of the roof of the skull, to form the longitudinal sinus. It was clear, therefore, that I had not erred in my interpretation, and that the posterior lobe of the brain of the Neanderthal man must have been as much flattened as I suspected it to be.

In truth, the Neanderthal cranium has most extraordinary characters. It has an extreme length of 8 inches, while its breadth is only 5.75 inches, or, in other words, its length is to its breadth as 100 : 72. It is exceedingly depressed, measuring only about 3.4 inches from the glabello-occipital line to the vertex. The longitudinal arc, measured

in the same way as in the Engis skull, is 12 inches; the transverse arc cannot be exactly ascertained in consequence of the absence of the temporal bones, but was probably about the same, and certainly exceeded 10½ inches. The horizontal circumference is 23 inches. But this great circumference arises largely from the vast development of the supraciliary ridges, though the perimeter of the brain case itself is not small. The large supraciliary ridges give the forehead a far more retreating appearance than its internal contour would bear out.

To an anatomical eye the posterior part of the skull is even more striking than the anterior. The occipital protuberance occupies the extreme posterior end of the skull, when the glabello-occipital line is made horizontal, and so far from any part of the occipital region extending beyond it, this region of the skull slopes obliquely upward and forward, so that the lambdoidal suture is situated well upon the upper surface of the cranium. At the same time, notwithstanding the great length of the skull, the sagittal suture is remarkably short (4½ inches), and the squamosal suture is very straight.

In reply to my questions, Dr. Fuhlrott writes that the occipital bone "is in a state of perfect preservation as far as the upper semicircular line, which is a very strong ridge, linear at its extremities, but enlarging toward the middle, where it forms two ridges (bourrelets), united by a linear continuation,

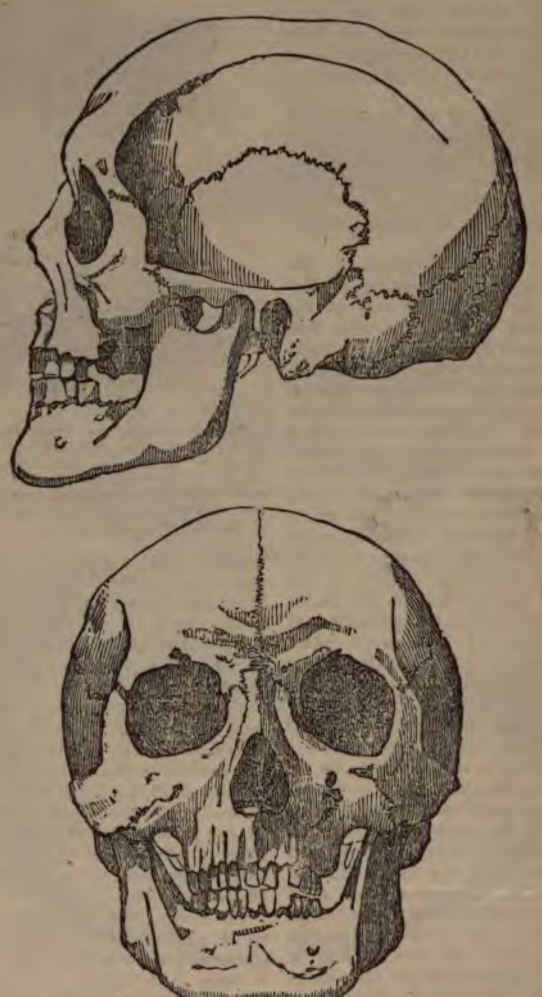


FIG. 26.—Side and front views of the round and orthognathous skull of a Calmuck, after Von Baer. One third the natural size.

which is slightly depressed in the middle.

"Below the left ridge the bone exhibits an obliquely inclined surface, six lines (French) long, and twelve lines wide."

This last must be the surface, the contour of which is shown in Fig 24 A, below *b*. It is particularly interesting, as it suggests that, notwithstanding the flattened condition of the occiput, the posterior cerebral lobes must have projected considerably beyond the cerebellum, and as it constitutes one among several points of similarity between the Neanderthal cranium and certain Australian skulls.

Such are the two best known forms of human cranium, which have been found in what may be fairly termed a fossil state. Can either be shown to fill up or diminish, to any appreciable extent, the structural interval which exists between man and the man-like apes? Or, on the other hand, does neither depart more widely from the average structure of the human cranium, than normally formed skulls of men are known to do at the present day?

It is impossible to form any opinion on these questions, without some preliminary acquaintance with the range of variation exhibited by human structure in general—a subject which has been but imperfectly studied, while even of what is known my limits will necessarily allow me to give only a very imperfect sketch.

The student of anatomy is perfectly well aware that there is not a single organ of the human body the structure of which does not vary, to a greater or less extent, in different individuals. The skeleton varies in the proportions, and even to a certain extent in the connections, of its constituent bones. The muscles which move the bones vary largely in their attachments. The varieties in the mode of distribution of the arteries are carefully classified, on account of the practical importance of a knowledge of their shiftings to the surgeon. The characters of the brain vary immensely, nothing being less constant than the form and size of the cerebral hemispheres, and the richness of the convolutions upon their surface, while the most changeable structures of all in the human brain are exactly those on which the unwise attempt has been made to base the distinctive characters of humanity, viz., the posterior cornu of the lateral ventricle, the hippocampus minor, and the degree of projection of the posterior lobe beyond the cerebellum. Finally, as all the world knows, the hair and skin of human beings may present the most extraordinary diversities in color and in texture.

So far as our present knowledge goes, the majority of the structural varieties to which allusion is here made are individual. The ape-like arrangement of certain muscles which is occasionally met with in the white races of mankind, is not known to be more common among Negroes or Australians; nor because the brain of the Hottentot Venus was found to be smoother, to have its convolutions more symmetrically *disposed*, and to be, so far, more ape-like than that of ordinary Europeans, are we justified in concluding a like condition of the brain to prevail universally among the lower races of mankind, however probable that conclusion may be.

We are, in fact, sadly wanting in information respecting the disposition of the soft and destructible organs of every race of mankind but our own; and even of the skeleton, our museums are lamentably deficient in every part but the cranium. Skulls enough there are, and since the time when Blumenbach and Camper first called attention to the marked and singular differences which they exhibit, skull-collecting and skull-measuring has been a zealously pursued branch of natural history, and the results obtained have been arranged and classified by various writers, among whom the late active and able Retzius must always be the first named.

Human skulls have been found to differ from one another, not merely in their absolute size and in the absolute capacity of the brain-case, but in the proportions which the diameters of the latter bear to one another: in the relative size of the bones of the face (and more particularly of the jaws and teeth) as compared with those of the skull; in the degree to which the upper jaw (which is of course followed by the lower) is thrown backward and downward under the forepart of the brain-case, or forward and upward in front of and beyond it. They differ further in the relations of the transverse diameter of the face, taken through the cheek-bones, to the transverse diameter of the skull; in the more rounded or more gable-like form of the roof of the skull, and in the degree to which the hinder part of the skull is flattened or projects beyond the ridge, into and below which the muscles of the neck are inserted.

In some skulls the brain-case may be said to be "*round*," the extreme length not exceeding the extreme breadth by a greater proportion than 100 to 80, while the difference may be much less. Men possessing such skulls were termed by Retzius "*brachycephalic*," and the skull of a Calmuck, of which a front and side view (reduced outline copies of which are given in Fig. 26) are depicted by Von Baer in his excellent "Crania selecta," affords a very admirable example of that kind of skull. Other skulls, such as that of a Negro copied in Fig. 27 from Mr. Busk's "Crania typica," have a very different, greatly elongated form, and may be termed "*oblong*." In this skull the extreme length is to the extreme breadth as 100 to not more than 67, and the transverse diameter of the human skull may fall below even this proportion. People having such skulls were called by Retzius "*dolichocephalic*."

The most cursory glance at the side views of these two skull will suffice to prove that they differ, in another respect, to a very striking extent. The profile of the face of the Calmuck is almost vertical, the facial bones being thrown downward and under the forepart of the skull. The profile of the face of a Negro, on the other hand, is singularly

inclined, the front part of the jaws projecting far forward beyond the level of the forepart of the skull. In the former case the skull is said to be "*orthognathous*" or straight-jawed; in the latter it is called "*prognathous*," a term which has been rendered, with more force than elegance, by the Saxon equivalent—"snouty."

Various methods have been devised in order to express with some accuracy the degree of prognathism or orthognathism of any given skull; most of these methods being essentially modifications of that devised by Peter Camper, in order to attain what he called the "facial angle."

But a little consideration will show that any "facial angle" that has been devised can be competent to express the structural modifications involved in prognathism and orthognathism, only in a rough and general sort of way. For the lines, the intersection of which forms the facial angle, are drawn through points of the skull, the position of each of which is modified by a number of circumstances, so that the angle obtained is a complex resultant of all these circumstances, and is not the expression of any one definite organic relation of the parts of the skull.

I have arrived at the conviction that no comparison of crania is worth very much that is not founded upon the establishment of a relatively fixed base line, to which the measurements, in all cases, must be referred. Nor do I think it is a very difficult matter to decide what that base line should be. The parts of the skull, like those of the rest of the animal framework, are developed in succession: the base of the skull is formed before its sides and roof; it is converted into cartilage earlier and more completely than the sides and roof; and the cartilaginous

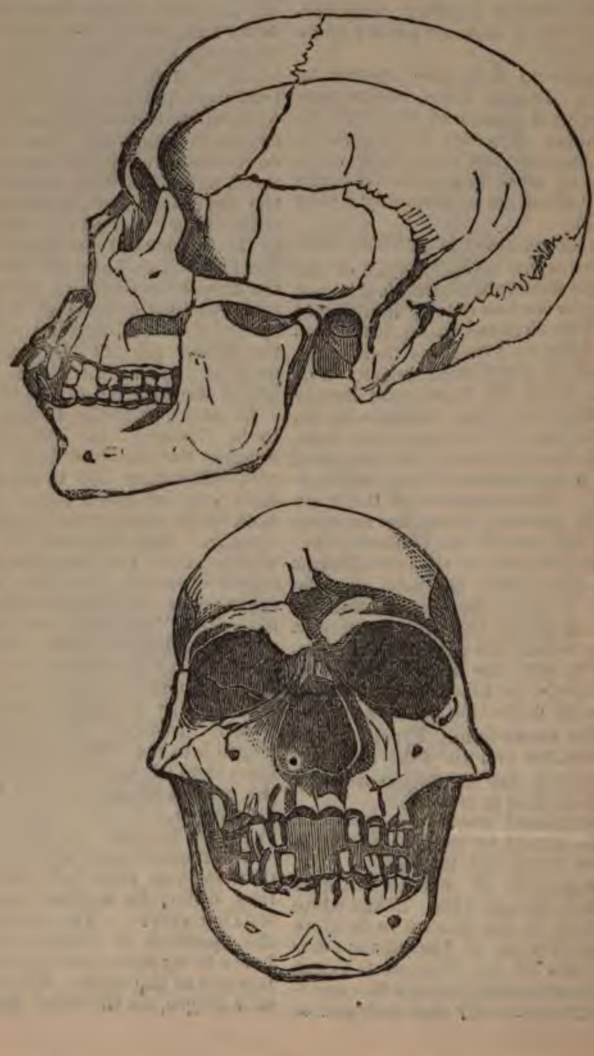


Fig. 27.—Oblong and prognathous skull of a Negro; side and front views one third of the natural size.

base ossifies, and becomes soldered into one piece long before the roof. I conceive then that the base of the skull may be demonstrated developmentally to be its relatively fixed part, the roof and sides being relatively movable.

The same truth is exemplified by the study of the modifications which the skull undergoes in ascending from the lower animals up to man.

In such a mammal as a beaver (Fig. 28), a line (*a. b.*) drawn through the bones termed basioccipital, basisphenoid, and presphenoid, is very long in proportion to the extreme length of the cavity which contains the cerebral hemispheres (*g. h.*) The plane of the occipital foramen (*b. c.*) forms a slightly acute angle with this "basicranial axis," while the plane of the tentorium (*i. T.*) is inclined at rather more than 90° to the "basicranial axis;" and so is the plane of the perforated plate (*a. d.*) by which the filaments of the olfactory nerve leave the skull. Again, a line drawn through the axis of the face, between the bones called ethmoid and vomer, the "basifacial axis" (*f. e.*), forms an exceedingly obtuse angle, where, when produced, it cuts the "basicranial axis."

If the angle made by the line *b. c.* with *a. b.*, be called the "occipital angle," and the angle made by the line *a. d.* with *a. b.* be termed the "olfactory angle," and that made by *i. T.* with *a. b.* the "tentorial angle," then all these, in the mammal in question, are nearly right angles, varying between 80° and 110°. The angle *e. f. b.*, or that made by the cranial with the facial axis, and which may be termed the "cranio-facial angle," is extremely obtuse, amounting, in the case of the beaver, to at least 150°.

But if a series of sections of mammalian skulls, intermediate between a Rodent and a Man (Fig. 28), be examined, it will be found that in the higher crania the basi-cranial axis becomes shorter relatively to the cerebral length; that the "olfactory angle" and "occipital angle" become more obtuse, and that the "cranio-facial angle," becomes more acute by the bending down, as it were, of the facial axis upon the cranial axis. At the same time the roof of the cranium becomes more and more arched, to allow of the increasing height of the cerebral hemispheres, which is eminently characteristic of man, as well as of that backward extension, beyond the cerebellum, which reaches it maximum in the South American monkeys. So that, at last, in the human skull (Fig. 29), the cerebral length is between twice and thrice as great as the length of the basicranial axis: the olfactory plane is 20° or 30° on the *under* side of that axis; the occipital angle, instead of being less than 90°, is as much as 150° or 160°; the cranio-facial angle may be 90° or

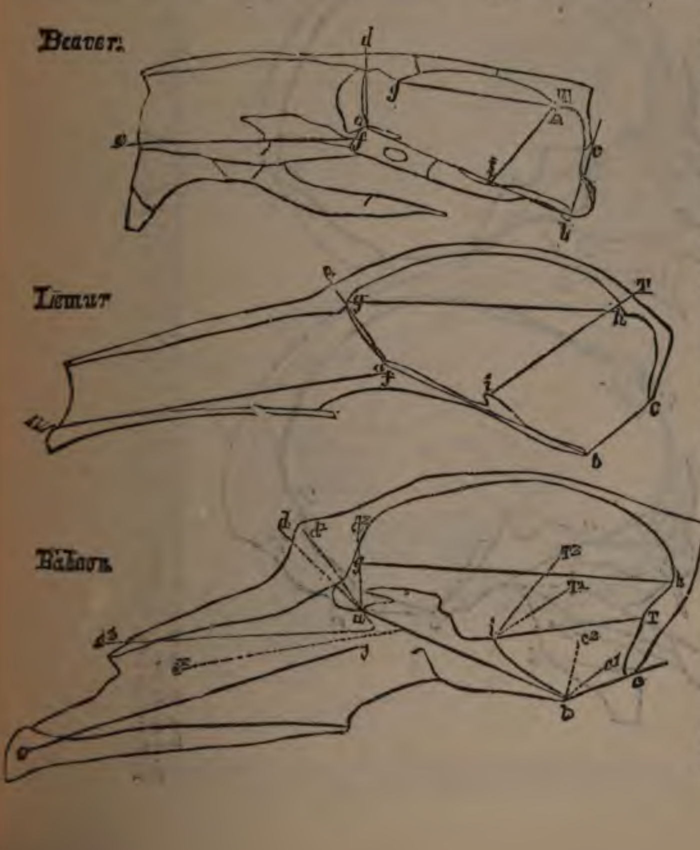


FIG. 28.—Longitudinal and vertical sections of the skulls of a Beaver (*Castor Canadensis*), a Lemur (*L. Catta*) and a Baboon (*Cynocephalus Porio*). *a b*, the basicranial axis; *b c*, the occipital plane; *i T*, the tentorial plane; *a d*, the olfactory plane; *f e*, the basifacial axis; *c b a*, occipital angle; *T i a*, tentorial angle; *d a b*, olfactory angle; *e f b*, cranio-facial angle; *g h*, extreme length of the cavity which lodges the cerebral hemispheres or "cerebral length." The length of the basicranial axis as to this length, or, in other words, the proportional length of the line *g h* to that of *a b* taken as 100, in the three skulls, is as follows: Beaver, 70 to 100; Lemur, 119 to 100; Baboon, 144 to 100. In an adult male Gorilla the cerebral length is as 170 to the basicranial axis taken as 100; in the Negro (Fig. 29) as 236 to 100; in the Constantinople skull (Fig. 29) as 266 to 100. The cranial difference between the highest Ape's skull and the lowest Man's is therefore very strikingly brought out by these measurements.

In the diagram of the Baboon's skull the dotted lines d^1 d^2, etc., give the angles of the Lemur's and Beaver's skull, as laid down upon the basicranial axis of the Baboon. The line *a b* has the same length in each diagram.

less, and the vertical height of the skull may have a large proportion to its length.

It will be obvious, from an inspection of the diagrams, that the basicranial axis is, in the ascending series of mammalia, a relatively fixed line, on which the bones of the sides and roof of the cranial cavity and of the face may be said to revolve downward and forward or backward, according to their position. The arc described by any one bone or plane, however, is not by any means always in proportion to the arc described by another.

Now comes the important question, can we discern between the lowest and the highest forms of the human cranium, anything answering, in however slight a degree, to this revolution of the side and roof bones of the skull upon the basicranial axis observed upon so great a scale in the mammalian series? Numerous observations lead me to believe that we must answer this question in the affirmative

The diagrams in Fig. 29 are reduced from very carefully made diagrams of sections of four skulls, two round and orthodnathous, two long and prognathous, taken longitudinally and vertically, through the middle. The sectional diagrams then have been superimposed in such a manner that the basal axes of the skulls coincide by their anterior ends and in their direction. The deviations of the rest of the contours (which represent the interior of the skulls only) show the differences of the skulls from one another when these axes are regarded as relatively fixed lines.

The dark contours are those of an Australian and of a Negro skull; the light contours are those of a Tartar skull, in the Museum of the Royal College of Surgeons; and of a well-developed round skull from a cemetery in Constantinople, of uncertain race, in my own possession.

It appears, at once, from these views, that the prognathous skulls, so far as their jaws are concerned, do really differ from the orthognathous in much the same way as, though to a far less degree than, the skulls of the lower mammals differ from those of Man. Furthermore, the plane of the occipital foramen (*b. c.*) forms a somewhat smaller angle with the axis in these particular prognathous skulls than in the orthogna-

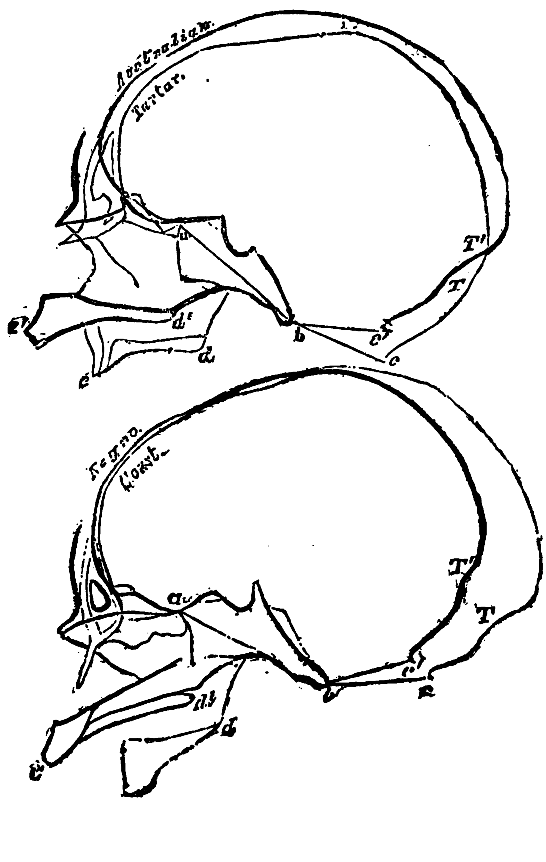


FIG. 29.—Sections of orthognathous (light contour) and prognathous (dark contour) skulls, one third of the natural size. *a b*, basicranial axis; *b c*, *b' c'*, plane of the occipital foramen; *d d'*, hinder end of the palatine bone; *e e'*, front end of the upper jaw; *T' T*, insertion of the tentorium.

thous; and the like may be slightly true of the perforated plate of the ethmoid—though this point is not so clear. But it is singular to remark that, in another respect, the prognathous skulls are less ape-like than the orthognathous, the cerebral cavity projecting decidedly more beyond the anterior end of the axis in the prognathous than in the orthognathous skulls.

It will be observed that these diagrams reveal an immense range of variation in the capacity and relative proportion to the cranial axis of the different regions of the cavity which contains the brain in the different skulls. Nor is the difference in the extent to which the cerebral overlaps the cerebellar cavity less singular. A round skull (Fig. 29, *Const.*) may have a greater posterior cerebral projection than a long one (Fig. 29, *Negro*).

Until human crania have been largely worked out in a manner similar to that here suggested—until it shall be an opprobrium to an ethnological collection to possess a single skull which is not bisected longitudinally—until the angles and measurements here mentioned, together with a number of others of which I cannot speak in this place, are determined and tabulated with reference to the basicranial axis, as unity, for large numbers of skulls of the different races of mankind, I do not think we shall have any very safe basis for that ethnological craniology which aspires to give the anatomical characters of the crania of the different races of mankind.

At present, I believe that the general outlines of what may be safely said upon that subject may be summed up in a very few words. Draw a line on a globe from the Gold Coast in Western Africa to the steppes of Tartary. At the southern and western end of that line there live the most dolichocephalic, prognathous, curly-haired, dark-skinned of men—the true Negroes. At the northern and eastern end of the same line there live the most brachycephalic, orthognathous, straight-haired, yellow-skinned of men—the Tartars and Calmucks. The two ends of this imaginary line are indeed, so to speak, ethnological antipodes. A line drawn at right angles, or nearly so, to this polar line through Europe and Southern Asia to Hindostan, would give us a sort of equator, around which round-headed, oval-headed, and oblong-headed, prognathous and orthognathous, fair and dark races—but none possessing the excessively marked characters of Calmuck or Negro—group themselves.

It is worthy of notice that the regions of the antipodal races are antipodal in climate, the greatest contrast the world affords, perhaps, being that between the damp, hot, steaming, alluvial coast plains of the West Coast of Africa and the arid, elevated steppes and plateaux of Central Asia, bitterly cold in winter, and as far from the sea as any part of the world can be.

From Central Asia eastward to the Pacific Islands and sub-continents on the one hand, and to America on the other, brachycephaly and orthognathism gradually diminish, and are replaced by dolichocephaly and prognathism, less, however, on the American Continent (throughout the whole length of which a rounded type of skull prevails largely, but not exclusively) than in the Pacific region, where, at length, on the Australian Continent and in the adjacent islands, the oblong skull, the projecting jaws, and the dark skin reappear; with so much departure, in other respects, from the Negro type, that ethnologists assign to these people the special title of "Negritoes."

The Australian skull is remarkable for its narrowness and for the thickness of its walls, especially in the region of the supraciliary

FIG. 30.—An Australian skull from Western Port, in the Museum of the Royal College of Surgeons, with the contour of the Neanderthal skull. Both reduced to one third the natural size.

ridge, which is frequently, though not by any means invariably, solid throughout, the frontal sinuses remaining undeveloped. The nasal depression, again, is extremely sudden, so that the brows overhang and give the countenance a particularly lowering, threatening expression. The occipital region of the skull, also, not unfrequently becomes less prominent; so that it not only fails to project beyond a line drawn perpendicular to the hinder extremity of the glabello-occipital line, but even, in some cases, begins to shelve away from it, forward, almost immediately. In consequence of this circumstance the parts of the occipital bone which lie above and below the tuberosity make a much more acute angle with one another than is usual, whereby the hinder part of the base of the skull appears obliquely truncated. Many Australian skulls have a considerable height, quite equal to that of the average of any other race, but there are others in which the cranial roof becomes remarkably depressed, the skull, at the same time, elongating so much that, probably, its capacity is not diminished. The majority of skulls possessing these characters which I have seen are from the neighborhood of Port Adelaide in South Australia, and have been used by the natives as water vessels; to which end the face has been knocked away, and a string passed through the vacuity and the occipital foramen, so that the skull was suspended by the greater part of its basis.

Fig. 30 represents the contour of a skull of this kind from Western Port, with the jaw attached, and of the Neanderthal skull, both reduced to one third of the size of nature. A small additional amount of flattening and lengthening, with a corresponding increase of the supraciliary ridge, would convert the Australian brain-case into a form identical with that of the aberrant fossil.

And now, to return to the fossil skulls, and to the rank which they occupy among, or beyond, these existing varieties of cranial conformation. In the first place, I must remark, that, as Professor Schmerling well observed (*supra*, p. 239) in commenting upon

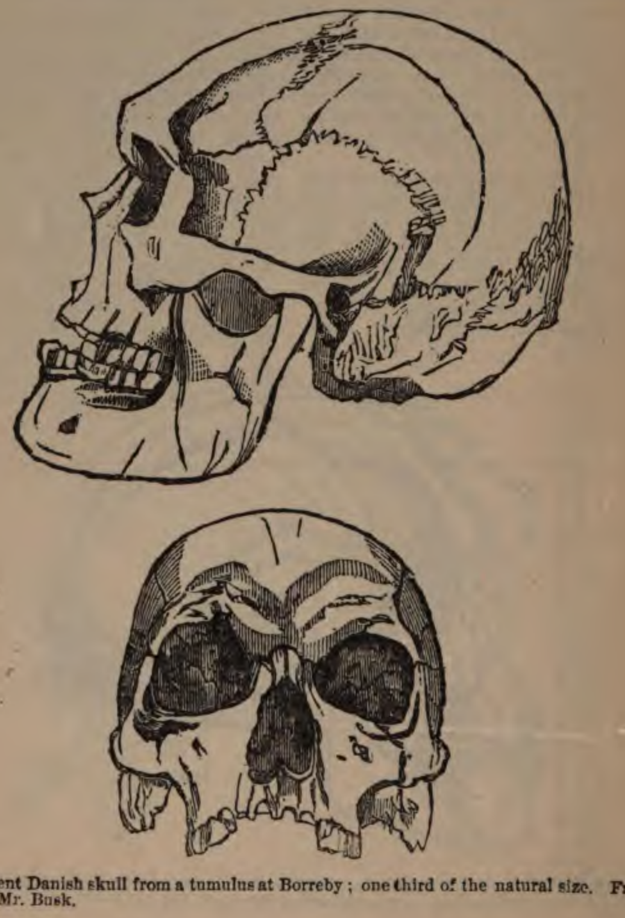


FIG. 31.—Ancient Danish skull from a tumulus at Borreby; one third of the natural size. From a camera lucida drawing by Mr. Busk.

the Engis skull, the formation of a safe judgment upon the question is greatly hindered by the absence of the jaws from both the crania, so that there is no means of deciding, with certainty, whether they were more or less prognathous than the lower existing races of mankind. And yet, as we have seen, it is more in this respect than any other, that human skulls vary, toward and from, the brutal type—the brain-case of an average dolichocephalic European differing far less from that of a Negro, for example, than his jaws do. In the absence of the jaws, then, any judgment on the relations of the fossil skulls to recent races must be accepted with a certain reservation.

But taking the evidence as it stands, and turning first to the Engis skull, I confess I can find no character in the remains of that cranium which, if it were a recent skull, would give any trustworthy clue as to the race to which it might appertain. Its contours and measurements agree very well with those of some Australian skulls which I have examined—and especially has it a tendency toward that occipital flattening, to the great extent of which, in some Australian skulls, I have alluded. But all Australian skulls do not present this flattening, and the supraciliary ridge of the Engis skull is quite unlike that of the typical Australians.

On the other hand, its measurements agree equally well with those of some European skulls. And assuredly, there is no mark of degradation about any part of its structure. It is, in fact, a fair average human skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage.

The case of the Neanderthal skull is very different. Under whatever aspect we view this cranium, whether we regard its vertical depression, the enormous thickness of its supraciliary ridges, its sloping occiput, or its long and straight squamosal suture, we meet with ape-like characters, stamping it as the most pithecoid of human crania yet discovered. But Professor Schaaffhausen states (*supra*, p. 241), that the cranium, in its present condition, holds 1033.24 cubic centimetres of water, or about 63 cubic inches, and as the entire skull could hardly have held less than an additional 12 cubic inches, its capacity may be estimated at about 75 cubic inches, which is the average capacity given by Morton for Polynesian and Hottentot skulls.

So large a mass of brain as this would alone suggest that the pithecoid tendencies, indicated by this skull, did not extend deep into the organization; and this conclusion is borne out by the dimensions of the other bones of the skeleton given by Professor Schaaffhausen, which show that the absolute height and relative proportions of the limbs were quite those of an European of middle stature. The bones are indeed stouter, but this and the great development of the muscular ridges noted by Dr. Schaaffhausen are characters to be expected in savages. The Patagonians, exposed without shelter or protection to a climate possibly not very dissimilar from that of Europe at the time during which the Neanderthal man lived, are remarkable for the stoutness of their limb bones.

In no sense, then, can the Neanderthal bones be regarded as the remains of a human being intermediate between Men and Apes. At most, they demonstrate the existence of a Man whose skull may be said to revert somewhat toward the pithecoid type—just as a Carrier, or a Pouter, or a Tumbler, may sometimes put on the plumage of its primitive stock, the *Columba livia*. And indeed, though truly the most pithecoid of known human skulls, the Neanderthal cranium is by no means so isolated as it appears to be at first, but forms, in reality, the extreme term of a series leading gradually from it to the highest and best developed of human crania. On the one hand, it is closely approached by the flattened Australian skulls, of which I have spoken, from which other Australian forms lead us gradually up to skulls having very much the type of the Engis cranium. And, on the other hand, it is even more closely affined to the skulls of certain ancient people who inhabited Denmark during the "stone period," and were probably either contemporaneous with, or later than, the makers of the "refuse heaps," or "Kjokkenmöddings" of that country.

The correspondence between the longitudinal contour of the Neanderthal skull and that of some of those skulls from the tumuli at Borreby, very accurate drawings of which have been made by Mr. Busk, is very close. The occiput is quite as retreating, the supraciliary ridges are nearly as prominent, and the skull is as low. Furthermore, the Borreby skull resembles the Neanderthal form more closely than any of the Australian skulls do, by the much more rapid retrocession of the forehead. On the other hand, the Borreby skulls are all somewhat broader, in proportion to their length, than the Neanderthal skull, while some attain that proportion of breadth to length (80 : 100) which constitutes brachycephaly.

In conclusion, I may say that the fossil remains of Man hitherto discovered do not seem to me to take us appreciably nearer to that lower pithecoid form, by the modification of which he has, probably, become what he is. And considering what is now known of the most ancient races of men; seeing that they fashioned flint axes and flint knives and bone-skewers of much the same pattern as those fabricated by the lowest savages at the present day, and that we have every reason to believe the habits and modes of living of such people to have remained the same from the time of the Mammoth and the tichorhine Rhinoceros till now, I do not know that this result is other than might be expected.

Where, then, must we look for primeval Man? Was the oldest Homo Sapiens pliocene

or miocene, or yet more ancient? In still older strata do the fossilized bones of an Ape more anthropoid or a Man more pithecoid than any yet known await the researches of some unborn paleontologist?

Time will show. But, in the mean while, if any form of the doctrine of progressive development is correct, we must extend by long epochs the most liberal estimate that has yet been made of the antiquity of man.

THE END.

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A COURSE OF SIX LECTURES.

BY

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LECTURE I.

THE PRESENT CONDITION OF ORGANIC NATURE.

WHEN it was my duty to consider what subject I would select for the six lectures which I shall now have the pleasure of delivering to you, it occurred to me that I could not do better than endeavor to put before you in a true light, or in what I might perhaps with more modesty call, that which I conceive myself to be the true light the position of a book which has been more praised and more abused, perhaps, than any book which has appeared for some years—I mean Mr. Darwin's work on the "Origin of Species." That work, I doubt not, many of you have read; for I know the inquiring spirit which is rife among you. At any rate, all of you will have heard of it—some by one kind of report and some by another kind of report; the attention of all and the curiosity of all have been probably more or less excited on the subject of that work. All I can do, and all I shall attempt to do, is to put before you that kind of judgment which has been formed by a man, who, of course, is liable to judge erroneously; but, at any rate, of one whose business and profession it is to form judgments upon questions of this nature.

And here, as it will always happen when dealing with an extensive subject, the greater part of my course—if, indeed, so small a number of lectures can be properly called a course—must be devoted to preliminary matters, or rather to a statement of those facts and of those principles which the work itself dwells upon, and brings more or less directly before us. I have no right to suppose that all or any of you are naturalists; and even if you were, the misconceptions and misunderstandings prevalent even among naturalists on these matters would make it desirable that I should take the course I now propose to take—that I should start from the beginning—that I should endeavor to point out what is the existing state of the organic world—that I should point out its past condition—that I should state what is the precise nature of the undertaking which Mr. Darwin has taken in hand;

that I should endeavor to show you what are the only methods by which that undertaking can be brought to an issue, and to point out to you how far the author of the work in question has satisfied those conditions, how far he has not satisfied them, how far they are satisfiable by man, and how far they are not satisfiable by man. And for to-night, in taking up the first part of this question, I shall endeavor to put before you a sort of broad notion of our knowledge of the condition of the living world. There are many ways of doing this. I might deal with it pictorially and graphically. Following the example of Humboldt in his "Aspects of Nature," I might endeavor to point out the infinite variety of organic life in every mode of its existence, with reference to the variations of climate and the like; and such an attempt would be fraught with interest to us all; but considering the subject before us, such a course would not be that best calculated to assist us. In an argument of this kind we must go farther and dig deeper into the matter; we must endeavor to look into the foundations of living nature, if I may so say, and discover the principles involved in some of her most secret operations. I propose, therefore, in the first place, to take some ordinary animal with which you are all familiar, and, by easily comprehensible and obvious examples drawn from it, to show what are the kind of problems which living beings in general lay before us; and I shall then show you that the same problems are laid open to us by all kinds of living beings. But, first, let me say in what sense I have used the words "organic nature." In speaking of the causes which lead to our present knowledge of organic nature, I have used it almost as an equivalent of the word "living," and for this reason—that in almost all living beings you can distinguish several distinct portions set apart to do particular things and work in a particular way. These are termed "organs," and the whole together is called "organic." And as it is universally characteristic of them, this term "organic" has been very conveniently employed to denote the whole of living nature—the whole of the plant world, and the whole of the animal world.

Few animals can be more familiar to you than that whose skeleton is shown on this diagram. You need not bother yourselves with this "*Equus caballus*" written under it; that is only the Latin name of it, and does not make it any better. It simply means the common horse. Suppose we wish to understand all about the horse. Our first object must be to study the structure of the animal. The whole of his body is inclosed within a hide, a skin covered with hair; and if that hide or skin be taken off, we find a great mass of flesh, or what is technically called muscle, being the substance which by its power of contraction enables the animal to move. These muscles move the hard parts one upon the other, and so give that strength and power of motion which renders the horse so useful to us in the performance of those services in which we employ them.

And then, on separating and removing the whole of this skin and flesh, you have a great series of bones, hard structures, bound together with ligaments, and forming the skeleton which is represented here.

In that skeleton there are a number of parts to be recognized. This long series of bones, beginning from the skull and ending in the tail, is called the spine, and these in front are the ribs; and then there are two pairs of limbs, one before and one behind; and these are what we all know as the fore-legs and the hind-legs. If we pursue our researches into the interior of this animal, we find within the framework of the skeleton a great cavity, or rather, I should say, two great cavities—one cavity beginning in the skull and running through the neck-bones, along the spine, and ending in the tail, containing the brain and the spinal marrow, which are extremely important organs. The second great cavity, commencing with the mouth, contains the gullet, the stomach, the long intestine, and all the rest of those internal apparatus which are essential for digestion; and then in the same great cavity there are lodged the heart and all the great vessels going from it; and, besides that, the organs of respiration — the lungs; and then the kidneys, and the organs of reproduction, and so on. Let us now endeavor to reduce this notion of a horse that we now have, to some such kind of simple expression as can be at once, and without difficulty, retained in the mind, apart from all minor details. If I make a transverse section, that is, if I were to saw a dead horse across, I should find that, if I left out the details, and supposing I took my section through the anterior region, and through the fore-limbs, I should have here this kind of section of the body (Fig. 1). Here would be the upper part of the animal—that great mass of bones that we spoke of as the spine (*a*, Fig. 1). Here I should have the alimentary canal (*b*, Fig. 1). Here I should have the heart (*c*, Fig. 1); and then you see, there would be a kind of double tube, the whole being inclosed within the hide; the spinal marrow would be placed in the upper tube (*a*, Fig. 1), and in the lower tube (*b*, Fig. 1) there would be the alimentary canal and the heart; and here I shall have the legs proceeding from each side. For simplicity's sake, I represent them merely as stumps (*e e*, Fig. 1). Now that is a horse—as mathematicians would say — reduced to its most simple expression. Carry that in your minds, if you please, as a simplified idea of the structure of the horse. The considerations which I have now put before you belong to what we technically call the "anatomy" of the horse. Now, suppose we go to work upon these several parts—flesh and hair, and skin and bone—and lay open these various organs with our scalpels, and examine them by means of our magnifying-glasses, and see what we can make of them. We shall find that the flesh is made up of bundles of strong fibres. The brain and nerves, too, we shall find, are made up of fibres, and these queer-looking things that are called

FIG. 1.

ganglionic corpuscles. If we take a slice of the bone and examine it, we shall find that it is very like this diagram of a section of the bone of an ostrich, though differing, of course, in some details; and if we take any part whatsoever of the tissue, and examine it, we shall find it all has a minute structure, visible only under the microscope. All these parts constitute microscopic anatomy or "Histology." These parts are constantly being changed; every part is constantly growing, decaying, and being replaced during the life of the animal. The tissue is constantly replaced by new material; and if you go back to the young state of the tissue in the case of muscle, or in the case of skin, or any of the organs I have mentioned, you will find that they all come under the same condition. Every one of these microscopic filaments and fibres (I now speak merely of the general character of the whole process)—every one of these parts—could be traced down to some modification of a tissue which can be readily divided into little particles of fleshy matter, of that substance which is composed of the chemical elements, carbon, hydrogen, oxygen, and nitrogen, having such a shape as this (Fig. 2). These particles, into which

FIG. 2.

all primitive tissues break up, are called cells. If I were to make a section of a piece of the skin of my hand, I should find that it was made up of these cells. If I examine the fibres which form the various organs of all living animals, I should find that all of them, at one time or other, had been formed out of a substance consisting of similar elements; so that you see, just as we reduced the whole body in the gross to that sort of simple expression given in Fig. 1, so we may reduce the whole of the microscopic structural elements to a form of even greater simplicity; just as the plan of the whole body may be so represented in a sense (Fig. 1), so the primary structure of every tissue may be represented by a mass of cells (Fig. 2).

Having thus, in this sort of general way, sketched to you what I may call, perhaps, the architecture of the body of the horse (what we term technically its morphology), I must now turn to another aspect. A horse is not a mere dead structure: it is an active, living, working machine. Hitherto we have, as it were, been looking at a steam-engine with the fires out, and nothing in the boiler; but the body of the living animal is a beautifully-formed active machine, and every part has its different work to do in the working of that machine, which is what we call its life. The horse, if you see him after his day's work is done, is cropping the grass in the fields, as it may be, or munching the oats in his stable. What is he doing? His jaws are working as a mill—and a very complex mill too—grinding the corn, or crushing the grass to a pulp. As soon as that operation has taken place, the food is passed down to the stomach, and there it is mixed with the chemical fluid called the gastric juice, a substance which has the peculiar property of making soluble and dissolving out the nutritious matter in the grass, and leaving behind those parts which are not nutritious; so that you have, first, the mill, then a sort of chemical digester; and then the food, thus partially dissolved, is carried back by the muscular contractions of the intestines into the hinder parts of the body, while the soluble portions are taken up into the blood. The blood is contained in a vast system of pipes, spreading through the whole body, connected with a force-pump—the heart—which, by its position and by the contractions of its valves, keeps the blood constantly circulating in one direction, never allowing it to rest; and then, by means of this circulation of the blood, laden as it is with the products of digestion, the skin, the flesh, the hair, and every other part of the body, draws from it that which it wants, and every one of these organs derives those materials which are necessary to enable it to do its work.

The action of each of these organs, the performance of each of these various duties, involve in their operation a continual absorption of the matters necessary for their support, from the blood, and a constant formation of waste products, which are returned to the blood, and conveyed by it to the lungs and the kidneys, which are organs that have allotted to them the office of extracting, separating, and getting rid of these waste products; and thus the general nourishment, labor, and repair of the whole machine is kept up with order and regularity. But not only is it a machine which feeds and appropriates to its own support the nourishment necessary to its existence—it is an engine

for locomotive purposes. The horse desires to go from one place to another; and to enable it to do this, it has those strong contractile bundles of muscles attached to the bones of its limbs, which are put in motion by means of a sort of telegraphic apparatus formed by the brain and the great spinal cord running through the spine or backbone; and to this spinal cord are attached a number of fibres termed nerves, which proceed to all parts of the structure. By means of these the eyes, nose, tongue, and skin—all the organs of perception—transmit impressions or sensations to the brain, which acts as a sort of great central telegraph office, receiving impressions and sending messages to all parts of the body, and putting in motion the muscles necessary to accomplish any movement that may be desired. So that here you have an extremely complex and beautifully-proportioned machine, with all its parts working harmoniously together toward one common object—the preservation of the life of the animal.

Now, note this: the horse makes up its waste by feeding, and its food is grass or oats, or perhaps other vegetable products; therefore, in the long run, the source of all this complex machinery lies in the vegetable kingdom. But where does the grass, or the oat, or any other plant, obtain this nourishing food producing material? At first it is a little seed, which soon begins to draw into itself from the earth and the surrounding air matters which in themselves contain no vital properties whatever; it absorbs into its own substance water, an inorganic body; it draws into its substance carbonic acid, an inorganic matter; and ammonia, another inorganic matter, found in the air; and then, by some wonderful chemical process, the details of which chemists do not yet understand, though they are near foreshadowing them, it combines them into one substance, which is known to us as "proteine," a complex compound of carbon, hydrogen, oxygen, and nitrogen, which alone possesses the property of manifesting vitality and of permanently supporting animal life. So that, you see, the waste products of the animal economy, the effete materials which are continually being thrown off by all living beings, in the form of organic matters, are constantly replaced by supplies of the necessary repairing and rebuilding materials drawn from the plants, which in their turn manufacture them, so to speak, by a mysterious combination of those same inorganic materials.

Let us trace out the history of the horse in another direction. After a certain time, as the result of sickness or disease, the effect of accident, or the consequence of old age, sooner or later, the animal dies. The multitudinous operations of this beautiful mechanism flag in their performance, the horse loses its vigor, and after passing through the curious series of changes comprised in its formation and preservation, it finally decays, and ends its life by going back into that inorganic world from which all but an inappreciable fraction of its substance was derived. Its bones become mere carbonate and phosphate of lime; the matter of its flesh, and of its other parts, becomes, in the long run, converted into carbonic acid, into water, and into ammonia. You will now perhaps understand the curious relation of the animal with the plant, of the organic with the inorganic world, which is shown in this diagram.

The plant gathers these inorganic materials together and makes them up into its own substance. The animal eats the plant and appropriates the nutritious portions to its own sustenance, rejects and gets rid of the useless matters; and, finally, the animal itself dies, and its whole body is decomposed and returned into the inorganic world. There is thus a constant circulation from one to the other, a continual formation of organic life from inorganic matters, and as constant a return of living bodies to the inorganic world; so that the materials of which our bodies are composed are largely, in all probability, the substances which constituted the matter of long extinct creations, but which have in the interval constituted a part of the inorganic world.

Thus we come to the conclusion, strange at first sight, that the matter constituting the living world is identical with that which forms the inorganic world. And not less true is it that, remarkable as are the powers, or, in other words, as are the forces which are exerted by living beings, yet all these forces are either identical with those which exist in the inorganic world, or they are convertible into them; I mean in just the same sense as the researches of physical philosophers have shown that heat is convertible into electricity, that electricity is convertible into magnetism, magnetism into mechanical force or chemical force, and any one of them into the other, each being measurable in terms of the other—even so, I say, that great law is applicable to the living world. Consider why is the skeleton of this horse capable of supporting the masses of flesh and the various organs forming the living body, unless it is because of the action of the same forces of cohesion which combines together the particles of matter composing this piece of chalk? What is there in the muscular contractile power of the animal but the force which is expressible, and which is in a certain sense convertible, into the force of gravity which it overcomes? Or, if you go to more hidden processes, in what does the process of digestion differ from those processes which are carried on in the laboratory of the chemist? Even if we take the most recondite and most complex operations of animal life—those of the nervous system, these of late years have been shown to be—I do not say identical in any sense with the electrical processes—but this has been shown, that they are in some way or other associated with them; that is to say, that every amount of nervous action is accompanied by a certain amount of electrical

disturbance in the particles of the nerves in which that nervous action is carried on. In this way the nervous action is related to electricity in the same way that heat is related to electricity; and the same sort of argument which demonstrates the two latter to be related to one another shows that the nervous forces are correlated to electricity; for the experiments of M. Dubois Reymond and others have shown that whenever a nerve is in a state of excitement, sending a message to the muscles or conveying an impression to the brain, there is a disturbance of the electrical condition of that nerve which does not exist at other times; and there are a number of other facts and phenomena of that sort; so that we come to the broad conclusion that not only as to living matter itself, but as to the forces that matter exerts, there is a close relationship between the organic and the inorganic world—the difference between them arising from the diverse combination and disposition of identical forces, and not from any primary diversity, so far as we can see.

I said just now that the horse eventually died and became converted into the same inorganic substances from whence all but an inappreciable fraction of its substance demonstrably originated, so that the actual wanderings of matter are as remarkable as the transmigrations of the soul fabled by Indian tradition. But before death has occurred, in the one sex or the other, and in fact in both, certain products or parts of the organism have been set free, certain parts of the organism of the two sexes have come into contact with one another, and from that conjunction from that union which then takes place, there results the formation of a new being. At stated times the mare, from a particular part of the interior of her body, called the ovary, gets rid of a minute particle of matter comparable in all essential respects with that which we called a cell a little while since, which cell contains a kind of nucleus in its centre, surrounded by a clear space and by a viscid mass of proteine substance (Fig. 2); and though it is different in appearance from the eggs which we are mostly acquainted with, it is really an egg. After a time this minute particle of matter, which may only be a small fraction of a grain in weight, undergoes a series of changes — wonderful, complex changes. Finally, upon its surface there is fashioned a little elevation, which afterward becomed divided and marked by a groove. The lateral boundaries of the groove extend upward and downward, and at length give rise to a double tube. In the upper smaller tube the spinal marrow and brain are fashioned; in the lower, the alimentary canal and heart, and at length two pairs of buds shoot out at the sides of the body, which are the rudiments of the limbs. In fact a true drawing of a section of the embryo in this state would in all essential respects resemble that diagram of a horse reduced to its simplest expression, which I first placed before you (Fig. 1).

Slowly and gradually these changes take place. The whole of the body at first can be broken up into "cells," which become in one place metamorphosed into muscle—in another place into gristle and bone — in another place into fibrous tissue—and in another into hair; every part becoming gradually and slowly fashioned, as if there were an artificer at work at each of these complex structures that we have mentioned. This embryo, as it is called, then passes into other conditions. This diagram represents the embryo of a dog; and I should tell you that there is a time when the embryos of neither dog, nor horse, nor porpoise, nor monkey, nor man, can be distinguished by any essential feature one from the other; there is a time when they each and all of them resemble this one of the dog. But as development advances, all the parts acquire their specialty, till at length you have the embryo converted into the form of the parent from which it started. So that you see, this living animal, this horse, begins its existence as a minute particle of nitrogenous matter, which, being supplied with nutriment (derived, as I have shown, from the inorganic world), grows up according to the special type and construction of its parents, works and undergoes a constant waste, and that waste is made good by nutriment derived from the inorganic world; the waste given off in this way being directly added to the inorganic world; and eventually the animal itself dies, and, by the process of decomposition, its whole body is returned to those conditions of inorganic matter in which its substance originated.

This, then, is that which is true of every living form, from the lowest plant to the highest animal—to man himself. You might define the life of every one in exactly the same terms as those which I have now used; the difference between the highest and the lowest being simply in the complexity of the developmental changes, the variety of the structural forms, the diversity of the physiological functions which are exerted by each.

If I were to take an oak tree as a specimen of the plant world, I should find that it originated in an acorn, which, too, commenced in a cell; the acorn is placed in the ground, and it very speedily begins to absorb the inorganic matters I have named, adds enormously to its bulk, and we can see it, year after year, extending itself upward and downward, attracting and appropriating to itself inorganic materials, which it vivifies, and eventually, as it ripens, gives off its own proper acorns, which again run the same course. But I need not multiply examples —from the highest to the lowest the essential features of life are the same, as I have described in each of these cases.

So much, then, for these particular features of the organic world, which you can understand and comprehend, so long as you confine yourself to one sort of living being, and study that only.

But, as you know, horses are not the only

Carbonic Acid. Water. Ammonia. Salines.

VEGETABLE WORLD. Fig. 3. ANIMAL WORLD.

living creatures in the world; and again, horses, like all other animals, have certain limits—are confined to a certain area on the surface of the earth on which we live—and, as that is the simpler matter, I may take that first. In its wild state, and before the discovery of America, when the natural state of things was interfered with by the Spaniards, the horse was only to be found in parts of the earth which are known to geographers as the Old World; that is to say, you might meet with horses in Europe, Asia, or Africa; but there were none in Australia, and there were none whatsoever in the whole continent of America, from Labrador down to Cape Horn. This is an empirical fact, and it is what is called, stated in the way I have given it you, the "geographical distribution" of the horse.

Why horses should be found in Europe, Asia, and Africa, and not in America, is not obvious; the explanation that the conditions of life in America are unfavorable to their existence, and that, therefore, they had not been created there, evidently does not apply; for when the invading Spaniards, or our own yeomen farmers, conveyed horses to these countries for their own use, they were found to thrive well and multiply very rapidly; and many are even now running wild in those countries, and in a perfectly natural condition. Now, suppose we were to do for every animal what we have here done for the horse—that is, to mark off and distinguish the particular district or region *to which each belonged*; and supposing we tabulated all these results, that would be called the geographical distribution of animals, while a corresponding study of plants would yield as a result the geographical distribution of plants.

I pass on from that now, as I merely wished to explain to you what I meant by the use of the term "geographical distribution." As I said, there is another aspect, and a much more important one, and that is, the relations of the various animals to one another. The horse is a very well-defined matter-of-fact sort of animal, and we are all pretty familiar with its structure. I dare say it may have struck you that it resembles very much no other member of the animal kingdom, except perhaps the zebra or the ass. But let me ask you to look along these diagrams. Here is the skeleton of the horse, and here the skeleton of the dog. You will notice that we have in the horse a skull, a backbone and ribs, shoulder-blades and haunch-bones. In the fore limb, one upper-arm bone, two fore-arm bones, wrist-bones (wrongly called knee), and middle hand-bones, ending in the three bones of a finger, the last of which is sheathed in the horny hoof of the fore-foot; in the hind-limb, one thigh-bone, two leg-bones, ankle-bones, and middle foot-bones, ending in the three bones of a toe, the last of which is incased in the hoof of the hind-foot. Now turn to the dog's skeleton. We find identically the same bones, but more of them, there being more toes in each foot, and hence more toe-bones.

Well, that is a very curious thing! The fact is that the dog and the horse—when one gets a look at them without the outward impediments of the skin—are found to be made in very much the same sort of fashion. And if I were to make a transverse section of the dog, I should find the same organs that I have already shown you as forming parts of the horse. Well, here is another skeleton—that of a kind of lemur—you see he has just the same bones; and if I were to make a transverse section of it, it would be just the same again. In your mind's eye turn him around, so as to put his backbone in a position inclined obliquely upward and forward, just as in the next three diagrams, which represent the skeletons of an orang, a chimpanzee, a gorilla, and you find you have no trouble in identifying the bones throughout; and lastly, turn to the end of the series, the diagram representing a man's skeleton, and still you find no great structural feature essentially altered. There are the same bones in the same relations. From the horse we pass on and on, with gradual steps, until we arrive at last at the highest known forms. On the other hand, take the other line of diagrams, and pass from the horse downward in the scale to this fish; and still, though the modifications are vastly greater, the essential framework of the organization remains unchanged. Here, for instance, is a porpoise; here is its strong backbone, with the cavity running through it, which con-

tains the spinal cord; here are the ribs, here the shoulder-blade; here is the little short upper-arm bone, here are the two fore-arm bones, the wrist-bone, and the finger-bones. Strange, is it not, that the porpoise should have in this queer-looking affair—its flapper (as it is called)—the same fundamental elements as the fore-leg of the horse or the dog, or the ape or man? and here you will notice a very curious thing—the hinder limbs are absent. Now, let us make another jump. Let us go to the codfish: here you see is the fore-arm, in this large pectoral fin—carrying your mind's eye onward from the flapper of the porpoise. And here you have the hinder limbs restored in the shape of these ventral fins. If I were to make a transverse section of this, I should find just the same organs that we have before noticed. So that, you see, there comes out this strange conclusion as the result of our investigations, that the horse, when examined and compared with other animals, is found by no means to stand alone in nature; but that there are an enormous number of other creatures which have backbones, ribs, and legs, and other parts arranged in the same general manner, and in all their formation exhibiting the same broad peculiarities.

I am sure that you cannot have followed me even in this extremely elementary exposition of the structural relations of animals, without seeing what I have been driving at all through, which is to show you that, step by step, naturalists have come to the idea of a unity of plan, or conformity of construction, among animals which appeared at first sight to be extremely dissimilar.

And here you have evidence of such a unity of plan among all the animals which have backbones, and which we technically call *Vertebrata*. But there are multitudes of other animals, such as crabs, lobsters, spiders, and so on, which we term *Annulosa*. In these I could not point out to you the parts that correspond with those of the horse—the backbone, for instance—as they are constructed upon a very different principle, which is also common to all of them; that is to say, the lobster, the spider, and the centipede have a common plan running through their whole arrangement, in just the same way that the horse, the dog, and the porpoise assimilate to each other.

Yet other creatures—whelks, cuttlefishes, oysters, snails, and all their tribe (*Mollusca*)—resemble one another in the same way, but differ from both *Vertebrata* and *Annulosa*; and the like is true of the animals called *Cœlenterata* (polypes) and *Protozoa* (animalcules and sponges).

Now by pursuing this sort of comparison, naturalists have arrived at the conviction that there are—some think five, and some seven—but certainly not more than the latter number—and perhaps it is simpler to assume five—distinct plans or constructions in the whole of the animal world; and that the hundreds of thousands of species of creatures on the surface of the earth are all reducible to those five, or, at most, seven, plans of organization.

But can we go no farther than that? When one has got so far, one is tempted to go on a step and inquire whether we cannot go back yet farther and bring down the whole to modifications of one primordial unit. The anatomist cannot do this; but if he call to his aid the study of development, he can do it. For we shall find that, distinct as those plans are, whether it be a porpoise or man or lobster, or any of those other kinds I have mentioned, every one begins its existence with one and the same primitive form—that of the egg, consisting, as we have seen, of a nitrogenous substance, having a small particle or nucleus in the centre of it. Furthermore, the earlier changes of each are substantially the same. And it is in this that lies that true "unity of organization" of the animal kingdom which has been guessed at and fancied for many years; but which it has been left to the present time to be demonstrated by the careful study of development. But is it possible to go another step farther still, and to show that in the same way the whole of the organic world is reducible to one primitive condition of form? Is there among the plants the same primitive form of organization, and is that identical with that of the animal kingdom? The reply to that question, too, is not uncertain or doubtful. It is now proved that every plant begins its existence under the same form; that is to say, in that of a cell—a particle of nitrogenous matter having substantially the same conditions. So that if you trace back the oak to its first germ, or a man, or a horse, or lobster, or oyster, or any other animal you choose to name, you shall find each and all of these commencing their existence in forms essentially similar to each other; and, furthermore, that the first processes of growth, and many of the subsequent modifications, are essentially the same in principle in almost all.

In conclusion, let me, in a few words, recapitulate the positions which I have laid down. And you must understand that I have not been talking mere theory; I have been speaking of matters which are as plainly demonstrable as the commonest propositions of Euclid—of facts that must form the basis of all speculations and beliefs in biological science. We have gradually traced down all organic forms, or, in other words, we have analyzed the present condition of animated nature, until we found that each species took its origin in a form similar to that under which all the others commence their existence. We have found the whole of the vast array of living forms, with which we are surrounded, constantly growing, increasing, decaying, and disappearing; the animal constantly attracting, modifying, and applying to its sustenance the matter of the vegetable kingdom, which derived its support from the absorption and conversion of inorganic matter. And so constant and universal is this absorption, waste, and repro

duction, that it may be said with perfect certainty that there is left in no one of our bodies at the present moment a millionth part of the matter of which they were originally formed! We have seen, again, that not only is the living matter derived from the inorganic world, but that the forces of that matter are all of them correlative with and convertible into those of inorganic nature.

This, for our present purposes, is the best view of the present condition of organic nature which I can lay before you; it gives you the great outlines of a vast picture, which you must fill up by your own study.

In the next lecture I shall endeavor in the same way to go back into the past, and to sketch in the same broad manner the history of life in epochs preceding our own.

LECTURE II.

THE PAST CONDITION OF ORGANIC NATURE.

In the lecture which I delivered last Monday evening I endeavored to sketch in a very brief manner, but as well as the time at my disposal would permit, the present condition of organic nature, meaning by that large title simply an indication of the great, broad, and general principles which are to be discovered by those who look attentively at the phenomena of organic nature as at present displayed. The general result of our investigations might be summed up thus: we found that the multiplicity of the forms of animal life, great as that may be, may be reduced to a comparatively few primitive plans or types of construction; that a further study of the development of those different forms revealed to us that they were again reducible, until we at last brought the infinite diversity of animal, and even vegetable life, down to the primordial form of a single cell.

We found that our analysis of the organic world, whether animals or plants, showed, in the long run, that they might both be reduced into, and were, in fact, composed of the same constituents. And we saw that the plant obtained the materials constituting its substance by a peculiar combination of matters belonging entirely to the inorganic world; that, then, the animal was constantly appropriating the nitrogenous matters of the plant to its own nourishment, and returning them back to the inorganic world, in what we spoke of as its waste; and that, finally, when the animal ceased to exist, the constituents of its body were dissolved and transmitted to that inorganic world whence they had been at first abstracted. Thus we saw in both the blade of grass and the horse, but the same elements differently combined and arranged. We discovered a continual circulation going on—the plant drawing in the elements of inorganic nature and combining them into food for the animal creation; the animal borrowing from the plant the matter for its own support, giving off during its life products which returned immediately to the inorganic world; and that eventually the constituent materials of the whole structure of both animals and plants were thus returned to their original source: there was a constant passage from one state of existence to another, and a returning back again.

Lastly, when we endeavored to form some notion of the nature of the forces exercised by living beings, we discovered that they—if not capable of being subjected to the same minute analysis as the constituents of those beings themselves—that they were correlative with—that they were the equivalents of the forces of inorganic nature—that they were, in the sense in which the term is now used, convertible with them. That was our general result.

And now, leaving the present, I must endeavor in the same manner to put before you the facts that are to be discovered in the past history of the living world, in the past conditions of organic nature. We have, to-night, to deal with the facts of that history—a history involving periods of time before which our mere human records sink into utter significance—a history the variety and physical magnitude of whose events cannot even be foreshadowed by the history of human life and human phenomena—a history of the most varied and complex character.

We must deal with the history, then, in the first place, as we should deal with all other histories. The historical student knows that his first business should be to inquire into the validity of his evidence, and the nature of the record in which the evidence is contained, that he may be able to form a proper estimate of the correctness of the conclusions which have been drawn from that evidence. So, here, we must pass, in the first place, to the consideration of a matter which may seem foreign to the question under discussion. We must dwell upon the nature of the records, and the credibility of the evidence they contain; we must look to the completeness or incompleteness of those records themselves, before we turn to that which they contain and reveal. The question of the credibility of the history, happily for us, will not require much consideration, for, in this history, unlike those of human origin, there can be no cavilling, no differences as to the reality and truth of the facts of which it is made up; the facts state themselves, and are laid out clearly before us.

But, although one of the greatest difficulties of the historical student is cleared out of our path, there are other difficulties—difficulties in rightly interpreting the facts as they are presented to us—which may be compared with the greatest difficulties of any other kinds of historical study.

What is this record of the past history of the globe, and what are the questions which are involved in an inquiry into its completeness or incompleteness? That record is composed of mud; and the question which we have to investigate this evening resolves itself into a question of the formation of mud. You may think, perhaps, that this is a vast step—of almost from the sublime to the

ridiculous—from the contemplation of the history of the past ages of the world's existence to the consideration of the history of the formation of mud! But, in nature, there is nothing mean and unworthy of attention; there is nothing ridiculous or contemptible in any of her works; and this inquiry, you will soon see, I hope, takes us to the very root and foundations of our subject.

How, then, is mud formed? Always, with some trifling exception, which I need not consider now—always, as the result of the action of water, wearing down and disintegrating the surface of the earth and rocks with which it comes in contact—pounding and grinding it down, and carrying the particles away to places where they cease to be disturbed by this mechanical action, and where they can subside and rest. For the ocean, urged by winds, washes, as we know, a long extent of coast, and every wave, loaded as it is with particles of sand and gravel as it breaks upon the shore, does something toward the disintegrating process. And thus, slowly but surely, the hardest rocks are gradually ground down to a powdery substance; and the mud thus formed, coarser or finer, as the case may be, is carried by the rush of the tides, or currents, till it reaches the comparatively deeper parts of the ocean, in which it can sink to the bottom, that is, to parts where there is a depth of about fourteen or fifteen fathoms, a depth at which the water is, usually, nearly motionless, and in which, of course, the finer particles of this detritus, or mud, as we call it, sinks to the bottom.

Or, again, if you take a river, rushing down from its mountain sources, brawling over the stones and rocks that intersect its path, loosening, removing, and carrying with it in its downward course the pebbles and lighter matters from its banks, it crushes and pounds down the rocks and earths in precisely the same way as the wearing action of the sea waves. The matters forming the deposit are torn from the mountain-side and whirled impetuously into the valley, more slowly over the plain, thence into the estuary, and from the estuary they are swept into the sea. The coarser and heavier fragments are obviously deposited first, that is, as soon as the current begins to lose its force by becoming amalgamated with the stiller depths of the ocean, but the finer and lighter particles are carried further on, and eventually deposited in a deeper and stiller portion of the ocean.

It clearly follows from this that mud gives us a chronology; for it is evident that supposing this, which I now sketch, to be the sea-bottom, and supposing this to be a coast-line; from the washing action of the sea upon the rock, wearing and grinding it down into a sediment of mud, the mud will be carried down, and at length deposited in the deeper parts of this sea-bottom, where it will form a layer; and then, while that first layer is hardening, other mud which is coming from the same source will, of course, be carried to the same place; and, as it is quite impossible for it to get beneath the layer already there, it deposits itself above it, and forms another layer, and in that way you gradually have layers of mud constantly forming and hardening one above the other, and conveying a record of time.

It is a necessary result of the operation of the law of gravitation that the uppermost layer shall be the youngest and the lowest the oldest, and that the different beds shall be older at any particular point or spot in exactly the ratio of their depth from the surface. So that if they were upheaved afterward, and you had a series of these different layers of mud, converted into sandstone, or limestone, as the case might be, you might be sure that the bottom layer was deposited first, and that the upper layers were formed afterward. Here, you see, is the first step in the history—these layers of mud give us an idea of time.

The whole surface of the earth—I speak broadly, and leave out minor qualifications—is made up of such layers of mud, so hard, the majority of them, that we call them rock, whether limestone or sandstone, or other varieties of rock. And, seeing that every part of the crust of the earth is made up in this way, you might think that the determination of the chronology, the fixing of the time which it has taken to form this crust, is a comparatively simple matter. To take a broad average, ascertain how fast the mud is deposited upon the bottom of the sea, or in the estuary of rivers; take it to be an inch or two, or three inches a year, or whatever you may roughly estimate it at, then take the total thickness of the whole series of stratified rocks, which geologists estimate at twelve or thirteen miles, or about seventy thousand feet, make a sum in short division, divide the total thickness by that of the quantity deposited in one year, and the result will, of course, give you the number of years which the crust has taken to form.

Truly, that looks a very simple process! It would be so except for certain difficulties, the very first of which is that of finding how rapidly sediments are deposited; but the main difficulty—a difficulty which renders any certain calculations of such a matter out of the question—is this, the sea-bottom on which the deposit takes place is continually shifting.

Instead of the surface of the earth being that stable, fixed thing that it is popularly believed to be, being, in common parlance, the very emblem of fixity itself, it is incessantly moving, and is, in fact, as unstable as the surface of the sea, except that its undulations are infinitely slower and enormously higher and deeper.

Now, what is the effect of this oscillation? Take the case to which I have previously referred. The finer or courser sediments that are carried down by the current of the river, will only be carried out a certain distance, and eventually, as we have already seen, on reaching the stiller part of the ocean, will be

FIG. 4.

deposited at the bottom.

Let C y (Fig. 4) be the sea-bottom, y D the shore, x y the sea-level, then the coarser deposit will subside over the region B, the finer over A, while beyond A there will be no deposit at all; and, consequently, no record will be kept, simply because no deposit is going on. Now, suppose that the whole land, C D, which we have regarded as stationary, goes down, as it does so, both A and B go farther out from the shore, which will be at y^1, x^1, y^1, being the new sea-level. The consequence will be that the layer of mud (A), being now, for the most part, further than the force of the current, is strong enough to convey even the finest *débris*, will, of course, receive no more deposits, and having attained a certain thickness will now grow no thicker.

We should be misled in taking the thickness of that layer, whenever it may be exposed to our view, as a record of time in the manner in which we are now regarding this subject, as it would give us only an imperfect and partial record it would seem to represent too short a period of time.

Suppose, on the other hand, that the land (C D) had gone on rising slowly and gradually—say an inch or two inches in the course of a century—what would be the practical effect of that movement? Why, that the sediment A and B which has been already deposited, would eventually be brought nearer to the shore-level, and again subjected to the wear and tear of the sea; and directly the sea begins to act upon it, it would of course soon cut up and carry it away, to a greater or less extent, to be re-deposited farther out.

Well, as there is, in all probability, not one single spot on the whole surface of the earth which has not been up and down in this way a great many times, it follows that the thickness of the deposits formed at any particular spot cannot be taken (even supposing we had at first obtained correct data as to the rate at which they took place), as affording reliable information as to the period of time occupied in its deposit. So that you see it is absolutely necessary from these facts, seeing that our record entirely consists of accumulations of mud superimposed one on the other; seeing in the next place that any particular spots on which accumulations have occurred have been constantly moving up and down, and sometimes out of the reach of a deposit, and at other times its own deposit broken up and carried away, it follows that our record must be in the highest degree imperfect, and we have hardly a trace left of thick deposits, or any definite knowledge of the area that they occupied in a great many cases. And mark this! That supposing even that the whole surface of the earth had been accessible to the geologist—that man had had access to every part of the earth, and had made sections of the whole, and put them all together—even then his record must of necessity be imperfect.

But to how much has man really access? If you will look at this map you will see that it represents the proportion of the sea to the earth: this colored part indicates all the dry land, and this other portion is the water. You will notice at once that the water covers three fifths of the whole surface of the globe, and has covered it in the same manner ever since man has kept any record of his own observations, to say nothing of the minute period during which he has cultivated geological inquiry. So that three fifths of the surface of the earth is shut out from us because it is under the sea. Let us look at the other two fifths, and see what are the countries in which anything that may be termed searching geological inquiry has been carried out: a good deal of France, Germany, and Great Britain and Ireland, bits of Spain, of Italy, and of Russia, have been examined, but of the whole great mass of Africa, except parts of the southern extremity, we know next to nothing; little bits of India, but of the greater part of the Asiatic continent nothing; bits of the Northern American States and of Canada, but of the greater part of the continent of North America, and in still larger proportion, of South America, nothing!

Under these circumstances, it follows that even with reference to that kind of imperfect information which we can possess, it is only about the ten thousandth part of the accessible parts of the earth that has been examined properly. Therefore, it is with justice that the most thoughtful of those who are concerned in these inquiries insist continually upon the imperfection of the geological record; for, I repeat, it is absolutely necessary, from the nature of things, that that record should

FIG. 5.

be of the most fragmentary and imperfect character. Unfortunately this circumstance has been constantly forgotten. Men of science, like young colts in a fresh pasture, are apt to be exhilarated on being turned into a new field of inquiry, and to go off at a hand-gallop, in total disregard of hedges and ditches, losing sight of the real limitation of their inquiries, and to forget the extreme imperfection of what is really known. Geologists have imagined that they could tell us what was going on at all parts of the earth's surface during a given epoch; they have talked of this deposit being contemporaneous with that deposit, until, from our little local histories of the changes at limited spots of the earth's surface, they have constructed a universal history of the globe as full of wonders and portents as any other story of antiquity.

But what does this attempt to construct a universal history of the globe imply? It implies that we shall not only have a precise knowledge of the events which have occurred at any particular point, but that we shall be able to say what events, at any one spot, took place at the same time with those at other spots.

Let us see now if that is in the nature of things practicable. Suppose that here I make a section of the Lake of Killarney, and here the section of another lake—that of Loch Lomond in Scotland, for instance. The rivers that flow into them are constantly carrying down deposits of mud, and beds, or strata are being as constantly formed, one above the other, at the bottom of those lakes. Now, there is not a shadow of doubt that in these two lakes the upper beds are all older than the lower—there is no doubt about that; but what does *this* tell us about the age of any given bed in Loch Lomond, as compared with that of any given bed in the Lake of Killarney? It is, indeed, obvious that if any two sets of deposits are separated and discontinuous, there is absolutely no means whatever given you by the nature of the deposit of saying whether one is much younger or older than the other; but you may say, as many have said and think, that the case is very much altered if the beds which we are comparing are continuous. Suppose two beds of mud hardened into rock—A and B are seen in section. (Fig. 5.)

Well, you say, it is admitted that the lowermost bed is always the older. Very well; B, therefore, is older than A. No doubt, *as a whole*, it is so; or if any parts of the two beds which are in the same vertical line are compared, it is so. But suppose you take what seems a very natural step farther, and say that the part *a* of the bed A is younger than the part *b* of the bed B. Is this sound reasoning? If you find any record of changes taking place at *b*, did they occur before any events which took place while *a* was being deposited? It looks all very plain sailing, indeed, to say that they did; and yet there is no proof of anything of the kind. As the former Director of this Institution, Sir H. De la Beche, long ago showed, this reasoning may involve an entire fallacy. It is extremely possible that *a* may have been deposited ages before *b*. It is very easy to understand how that can be. To return to Fig. 4; when A and B were deposited, they were *substantially* contemporaneous; A being simply the finer deposit, and B the coarser of the same detritus or waste of land. Now suppose that the sea-bottom goes down (as shown in Fig. 4), so that the first deposit is carried no farther than *a*, forming the bed A^1, and the coarse no farther than *b*, forming the bed B^1, the result will be the formation of two continuous beds, one of fine sediment (AA^1) overlapping another of course sediment (BB^1). Now suppose the whole sea-bottom is raised up, and a section exposed about the point A^1; no doubt, *at this spot*, the upper bed is younger than the lower. But we should obviously greatly err if we concluded that the mass of the upper bed at A was younger than the lower bed at B; for we have just seen that they are contemporaneous deposits. Still more should we be in error if we supposed the upper bed at A to be younger than the continuation of the lower bed at B^1; for A was deposited long before B^1. In fine, if, instead of comparing immediately adjacent parts of two beds, one of which lies upon another, we compare distant parts, it is quite possible that the upper may be any number of years older than the under, and the under any number of years younger than the upper.

Now you must not suppose that I put this before you for the purpose of raising a paradoxical difficulty; the fact is, that the great mass of deposits have taken place in sea-bottoms which are gradually sinking, and have been formed under the very conditions I am here supposing.

Do not run away with the notion that this subverts the principle I laid down at first.

The error lies in extending a principle which is perfectly applicable to deposits in the same vertical line to deposits which are not in that relation to one another.

It is in consequence of circumstances of this kind, and of others that I might mention to you, that our conclusions on and interpretations of the record are really and strictly only valid so long as we confine ourselves to one vertical section. I do not mean to tell you that there are no qualifying circumstances, so that, even in very considerable areas, we may safely speak of conformably superimposed beds being older or younger than others at many different points. But we can never be quite sure in coming to that conclusion, and especially we cannot be sure if there is any break in their continuity, or any very great distance between the points to be compared.

Well, now, so much for the record itself—so much for its imperfections—so much for the conditions to be observed in interpreting it, and its chronological indications, the moment we pass beyond the limits of a vertical linear section.

Now let us pass from the record to that which it contains—from the book itself to the writing and the figures on its pages. This writing and these figures consist of remains of animals and plants which, in the great majority of cases, have lived and died in the very spot in which we now find them, or at least in the immediate vicinity. You must all of you be aware—and I referred to the fact in last Monday's lecture—that there are vast numbers of creatures living at the bottom of the sea. These creatures, like all others, sooner or later die, and their shells and hard parts lie at the bottom; and then the fine mud which is being constantly brought down by rivers and the action of the wear and tear of the sea, covers them over and protects them from any further change or alteration; and, of course, as in process of time the mud becomes hardened and solidified, the shells of these animals are preserved and firmly imbedded in the limestone or sandstone which is being thus formed. You may see in the galleries of the Museum up-stairs specimens of limestones in which such fossil remains of existing animals are imbedded. There are some specimens in which turtles' eggs have been imbedded in calcareous sand, and before the sun had hatched the young turtles they became covered over with calcareous mud, and thus have been preserved and fossilized.

Not only does this process of imbedding and fossilization occur with marine and other aquatic animals and plants, but it affects those land animals and plants which are drifted away to sea, or become buried in bogs or morasses; and the animals which have been trodden down by their fellows and crushed in the mud at the river's bank, as the herd have come to drink. In any of these cases the organisms may be crushed or be mutilated, before or after putrefaction, in such a manner that perhaps only a part will be left in the form in which it reaches us. It is, indeed, a most remarkable fact, that it is quite an exceptional case to find a skeleton of any one of all the thousands of wild land animals that we know are constantly being killed, or dying in the course of nature: they are preyed on and devoured by other animals, or die in places where their bodies are not afterward protected by mud. There are other animals existing in the sea, the shells of which form exceedingly large deposits. You are probably aware that before the attempt was made to lay the Atlantic telegraphic cable, the Government employed vessels in making a series of very careful observations and soundings of the bottom of the Atlantic; and although, as we must all regret, that up to the present time that project has not succeeded, we have the satisfaction of knowing that it yielded some most remarkable results to science. The Atlantic Ocean had to be sounded right across, to depths of several miles in some places, and the nature of its bottom was carefully ascertained. Well, now, a space of about 1000 miles wide from east to west, and I do not exactly know how many from north to south, but at any rate 600 or 700 miles, was carefully examined, and it was found that over the whole of that immense area an excessively fine chalky mud is being deposited; and this deposit is entirely made up of animals whose hard parts are deposited in this part of the ocean, and are doubtless gradually acquiring solidity and becoming metamorphosed into a chalky limestone. Thus, you see, it is quite possible in this way to preserve unmistakable records of animal and vegetable life. Whenever the sea-bottom, by some of those undulations of the earth's crust that I have referred to, becomes upheaved, and sections or borings are made, or pits are dug, then we become able to examine the contents and constituents of these ancient sea-bottoms, and find out what manner of animals lived at that period.

Now it is a very important consideration in its bearings on the completeness of the record, to inquire how far the remains contained in these fossiliferous limestones are able to convey anything like an accurate or complete account of the animals which were in existence at the time of its formation. Upon that point we can form a very clear judgment, and one in which there is no possible room for any mistake. There are of course a great number of animals—such as jelly-fishes and other animals—without any hard parts, of which we cannot reasonably expect to find any traces whatever: there is nothing of them to preserve. Within a very short time, you will have noticed, after they are removed from the water, they dry up to a mere nothing; certainly they are not of a nature to leave any very visible traces of their existence on such bodies as chalk or mud. Then, again, look at land animals; it is, as I have said, a very uncommon thing to find a land animal entire after death. Insects and other carnivorous animals very speedily pull them to pieces, putrefaction takes place.

and so, out of the hundreds of thousands that are known to die every year, it is the rarest thing in the world to see one imbedded in such a way that its remains would be preserved for a lengthened period. Not only is this the case, but even when animal remains have been safely imbedded, certain natural agents may wholly destroy and remove them. Almost all the hard parts of animals—the bones and so on—are composed chiefly of phosphate of lime and carbonate of lime. Some years ago I had to make an inquiry into the nature of some very curious fossils sent to me from the north of Scotland. Fossils are usually hard bony structures that have become imbedded in the way I have described, and have gradually acquired the nature and solidity of the body with which they are associated; but in this case I had a series of *holes* in some pieces of rock, and nothing else. These holes, however, had a certain definite shape about them, and when I got a skilful workman to make castings of the interior of these holes, I found that they were the impressions of the joints of a backbone and of the armor of a great reptile, twelve or more feet long. This great beast had died and got buried in the sand, the sand had gradually hardened over the bones, but remained porous. Water had trickled through it, and that water being probably charged with a superfluity of carbonic acid, had dissolved all the phosphate and carbonate of lime, and the bones themselves had thus decayed and entirely disappeared; but as the sandstone happened to have consolidated by that time, the precise shape of the bones was retained. If that sandstone had remained soft a little longer, we should have known nothing whatsoever of the existence of the reptile whose bones it had incased.

How certain it is that a vast number of animals which have existed at one period on this earth have entirely perished, and left no trace whatever of their forms, may be proved to you by other considerations. There are large tracts of sandstone in various parts of the world, in which no body has yet found anything but footsteps. Not a bone of any description, but an enormous number of traces of footsteps. There is no question about them. There is a whole valley in Connecticut covered with these footsteps, and not a single fragment of the animals which made them have yet been found. Let me mention another case while upon that matter, which is even more surprising than those to which I have yet referred. There is a limestone formation near Oxford, at a place called Stonesfield, which has yielded the remains of certain very interesting mammalian animals, and up to this time, if I recollect rightly, there have been found seven specimens of its lower jaws, and not a bit of anything else, neither limb-bones nor skull, or any part whatever; not a fragment of the whole system! Of course, it would be preposterous to imagine that the beasts had nothing else but a lower jaw! The probability is, as Dr Buckland showed, as the result of his observations on dead dogs in the river Thames, that the lower jaw, not being secured by very firm ligaments to the bones of the head, and being a weighty affair, would easily be knocked off, or might drop away from the body as it floated in water in a state of decomposition. The jaw would thus be deposited immediately, while the rest of the body would float and drift away altogether, ultimately reaching the sea, and perhaps becoming destroyed. The jaw becomes covered up and preserved in the river silt, and thus it comes that we have such a curious circumstance as that of the lower jaws in the Stonesfield slates. So that, you see, faulty as these layers of stone in the earth's crust are, defective as they necessarily are as a record, the account of contemporaneous vital phenomena presented by them is, by the necessity of the case, infinitely more defective and fragmentary.

It was necessary that I should put all this very strongly before you, because, otherwise, you might have been led to think differently of the completeness of our knowledge by the next facts I shall state to you.

The researches of the last three quarters of a century have, in truth, revealed a wonderful richness of organic life in those rocks. Certainly not fewer than thirty or forty thousand different species of fossils have been discovered. You have no more ground for doubting that these creatures really lived and died at or near the places in which we find them than you have for like scepticism about a shell on the sea-shore. The evidence is as good in the one case as in the other.

Our next business is to look at the general character of these fossil remains, and it is a subject which will be requisite to consider carefully; and the first point for us is to examine how much the extinct *Flora* and *Fauna* as a *whole*—disregarding altogether the *succession* of their constituents, of which I shall speak afterward—differ from the *Flora* and *Fauna* of the present day; how far they differ in what we *do* know about them, leaving altogether out of consideration speculations based on what we *do not* know.

I strongly imagine that if it were not for the peculiar appearance that fossilized animals have, that any of you might readily walk through a museum which contains fossil remains mixed up with those of the present forms of life, and I doubt very much whether your uninstructed eyes would lead you to see any vast or wonderful difference between the two. If you looked closely, you would notice, in the first place, a great many things very like animals with which you are acquainted now: you would see differences of shape and proportion, but on the whole a close similarity.

I explained what I meant by Orders the other day, when I described the animal kingdom as being divided into sub-kingdoms, classes, and orders. If you divide the animal kingdom into orders, you will find that there are above one hundred and twenty. The number may vary on one side or the

other, but this is a fair estimate. That is the sum total of the orders of all the animals which we know now, and which have been known in past times, and left remains behind.

Now, how many of those are absolutely extinct? That is to say, how many of these orders of animals have lived at a former period of the world's history, but have at present no representatives? That is the sense in which I meant to use the word "extinct." I mean that those animals did live on this earth at one time, but have left no one of their kind with us at the present moment. So that estimating the number of extinct animals is a sort of way of comparing the past creation as a whole with the present as a whole. To make that clear, I have written in red ink on these diagrams the names of all those extinct orders, and in black ink the names of the rest. Among the mammalia and birds there are none extinct; but when we come to the reptiles there is a most wonderful thing: out of the eight orders, or thereabouts, which you can make among reptiles, one half are extinct. These diagrams of the plesiosaurus, the ichthyosaurus, the pterodactyle, give you a notion of some of these extinct reptiles. And here is the cast of the pterodactyle and bones of the ichthyosaurus and the plesiosaurus, just as fresh as if it had been recently dug up in a churchyard. Thus, in the reptile class, there are no less than half of the orders which are absolutely extinct. If we turn to the *Amphibia*, there was one extinct order, the labyrinthodonts, typified by the large salamander-like beast shown in this diagram.

No order of fishes is known to be extinct. Every fish that we find in the strata—to which I have been referring—can be identified and placed in one of the orders which exist at the present day. There is not known to be a single ordinal form of insect extinct. There are only two orders extinct among the *Crustacea*. There is not known to be an extinct order of these creatures, the parasitic and other worms; but there are two, not to say three, absolutely extinct orders of this class, the *Echinodermata;* out of all the orders of the *Cœlenterata* and *Protozoa*, only one, the Rugose Corals.

So that, you see, out of somewhere about 120 orders of animals, taking them altogether, you will not, at the outside estimate, find above ten or a dozen extinct. Summing up all the order of animals which have left remains behind them, you will not find above ten or a dozen which cannot be arranged with those of the present day; that is to say, that the difference does not amount to much more than ten per cent; and the proportion of extinct orders of plants is still smaller. I think that that is a very astounding, a most astonishing fact, seeing the enormous epochs of time which have elapsed during the constitution of the surface of the earth as it at present exists; it is, indeed, a most astounding thing that the proportion of extinct ordinal types should be so exceedingly small.

But now, there is another point of view in which we must look at this past creation. Suppose that we were to sink a vertical pit through the floor beneath us, and that I could succeed in making a section right through in the direction of New Zealand, I should find in each of the different beds through which I passed the remains of animals which I should find in that stratum and not in the others. First, I should come upon beds of gravel or drift containing the bones of large animals, such as the elephant, rhinoceros, and cave tiger. Rather curious things to fall across in Piccadilly! If I should dig lower still, I should come upon a bed of what we call the London clay, and in this, as you will see in our galleries up-stairs, are found remains of strange cattle, remains of turtles, palms, and large tropical fruits; with shellfish such as you see the like of now only in tropical regions. If I went below that, I should come upon the chalk, and there I should find something altogether different, the remains of ichthyosauri and pterodactyles, and ammonites, and so forth.

I do not know what Mr. Godwin Austin would say comes next, but probably rocks containing more ammonites, and more ichthyosauri and plesiosauri, with a vast number of other things; and under that I should meet with yet older rocks, containing numbers of strange shells and fishes; and in thus passing from the surface to the lowest depths of the earth's crust, the forms of animal life and vegetable life which I should meet with in the successive beds would, looking at them broadly, be the more different the farther that I went down. Or, in other words, inasmuch as we started with the clear principle, that in a series of naturally-disposed mud beds the lowest are the oldest, we should come to this result, that the farther we go back in time the more difference exists between the animal and vegetable life of an epoch and that which now exists. That was the conclusion to which I wished to bring you at the end of this lecture.

LECTURE III.

THE METHOD BY WHICH THE CAUSES OF THE PRESENT AND PAST CONDITIONS OF ORGANIC NATURE ARE TO BE DISCOVERED.—THE ORIGINATION OF LIVING BEINGS.

In the two preceding lectures I have endeavored to indicate to you the extent of the subject-matter of the inquiry upon which we are engaged; and now, having thus acquired some conception of the past and present phenomena of organic nature, I must turn, to-night, to that which constitutes the great problem which we have set before ourselves—I mean, the question of what knowledge we have of the causes of these phenomena of organic nature, and how such knowledge is obtainable.

Here, on the threshold of inquiry, an objection meets us. There are in the world a number of extremely worthy, well-meaning persons, whose judgments and opinions are

entitled to the utmost respect on account of their sincerity, who are of opinion that vital phenomena, and especially all questions relating to the origin of vital phenomena, are questions quite apart from the ordinary run of inquiry, and are, by their very nature, placed out of our reach. They say that all these phenomena originated miraculously, or in some way totally different from the ordinary course of nature, and that therefore they conceive it to be futile, not to say presumptuous, to attempt to inquire into them.

To such sincere and earnest persons, I would only say, that a question of this kind is not to be shelved upon theoretical or speculative grounds. You may remember the story of the sophist who demonstrated to Diogenes in the most complete and satisfactory manner that he could not walk; that, in fact, all motion was an impossibility; and that Diogenes refuted him by simply getting up and walking round his tub. So, in the same way, the man of science replies to objections of this kind, by simply getting up and walking onward, and showing what science has done and is doing—by pointing to that immense mass of facts which have been ascertained and systematized under the forms of the great doctrines of morphology, of development, of distribution, and the like. He sees an enormous mass of facts and laws relating to organic beings, which stand on the same good sound foundation as every other natural law; and, therefore, with this mass of facts and laws before us, seeing that, as far as organic matters have hitherto been accessible and studied, they have shown themselves capable of yielding to scientific investigation, we may accept this as proof that order and law reign there as well as in the rest of nature; and the man of science says nothing to objectors of this sort, but supposes that we can and shall walk to the origin of organic nature, in the same way that we have walked to a knowledge of the laws and principles of the inorganic world.

But there are objectors who say the same from ignorance and ill-will. To such I would reply that the objection comes ill from them, and that the real presumption, I may almost say the real blasphemy, in this matter, is in the attempt to limit that inquiry into the causes of phenomena which is the source of all human blessings, and from which has sprung all human prosperity and progress; for, after all, we can accomplish comparatively little; the limited range of our own faculties bounds us on every side—the field of our powers of observation is small enough, and he who endeavors to narrow the sphere of our inquiries is only pursuing a course that is likely to produce the greatest harm to his fellow-men.

But now, assuming, as we all do, I hope, that these phenomena are properly accessible to inquiry, and setting out upon our search into the causes of the phenomena of organic nature, or, at any rate, setting out to discover how much we at present know upon these abstruse matters, the question arises as to what is to be our course of proceeding, and what method we must lay down for our guidance. I reply to that question, that our method must be exactly the same as that which is pursued in any other scientific inquiry, the method of scientific investigation being the same for all orders of facts and phenomena whatsoever.

I must dwell a little on this point, for I wish you to leave this room with a very clear conviction that scientific investigation is not, as many people seem to suppose, some kind of modern black art. I say that you might easily gather this impression from the manner in which many persons speak of scientific inquiry, or talk about inductive and deductive philosophy, or the principles of the "Baconian philosophy." I do protest that, of the vast number of cants in this world, there are none, to my mind, so contemptible as the pseudo-scientific cant which is talked about the "Baconian philosophy."

To hear people talk about the great chancellor—and a very great man he certainly was—you would think that it was he who had invented science, and that there was no such thing as sound reasoning before the time of Queen Elizabeth! Of course, you say, that cannot possibly be true; you perceive, on a moment's reflection, that such an idea is absurdly wrong; and yet, so firmly rooted is this sort of impression—I cannot call it an idea or conception—the thing is too absurd to be entertained—but so completely does it exist at the bottom of most men's minds, that this has been a matter of observation with me for many years past. There are many men who, though knowing absolutely nothing of the subject with which they may be dealing, wish, nevertheless, to damage the author of some view with which they think fit to disagree. What they do, then, is not to go and learn something about the subject, which one would naturally think the best way of fairly dealing with it; but they abuse the originator of the view they question, in a general manner, and wind up by saying that, "After all, you know, the principles and method of this author are totally opposed to the canons of the Baconian philosophy." Then everybody applauds, as a matter of course, and agrees that it must be so. But if you were to stop them all in the middle of their applause, you would probably find that neither the speaker nor his applauders could tell you how or in what way it was so, neither the one nor the other having the slightest idea of what they mean when they speak of the "Baconian philosophy."

You will understand, I hope, that I have not the slightest desire to join in the outcry against either the morals, the intellect, or the great genius of Lord Chancellor Bacon. He was undoubtedly a very great man, let people say what they will of him; but notwithstanding all that he did for philosophy, it would be entirely wrong to suppose that the methods of modern scientific inquiry originated with him, or with his age; they originated with the first man, whoever he was; and

indeed existed long before him, for many of the essential processes of reasoning are exerted by the higher order of brutes as completely and effectively as by ourselves. We see in many of the brute creation the exercise of one, at least, of the same powers of reasoning as that which we ourselves employ.

The method of scientific investigation is nothing but the expression of the necessary mode of working of the human mind. It is simply the mode at which all phenomena are reasoned about, rendered precise and exact. There is no more difference, but there is just the same kind of difference, between the mental operations of a man of science and those of an ordinary person, as there is between the operations and methods of a baker or of a butcher weighing out his goods in common scales, and the operations of a chemist in performing a difficult and complex analysis by means of his balance and finely-graduated weights. It is not that the action of the scales in the one case, and the balance in the other, differ in the principles of their construction or manner of working; but the beam of one is set on an infinitely finer axis than the other, and of course turns by the addition of a much smaller weight.

You will understand this better, perhaps, if I give you some familiar example. You have all heard it repeated, I dare say, that men of science work by means of induction and deduction, and that by the help of these operations, they, in a sort of sense, wring from nature certain other things, which are called natural laws and causes, and that out of these, by some cunning skill of their own, they build up hypotheses and theories. And it is imagined by many that the operations of the common mind can be by no means compared with these processes, and that they have to be acquired by a sort of special apprenticeship to the craft. To hear all these large words you would think that the mind of a man of science must be constituted differently from that of his fellow-men; but if you will not be frightened by terms, you will discover that you are quite wrong, and that all these terrible apparatus are being used by yourselves every day and every hour of your lives.

There is a well-known incident in one of Molière's plays, where the author makes the hero express unbounded delight on being told that he had been talking prose during the whole of his life. In the same way, I trust that you will take comfort, and be delighted with yourselves, on the discovery that you have been acting on the principles of inductive and deductive philosophy during the same period. Probably there is not one here to-night who has not in the course of the day had occasion to set in motion a complex train of reasoning, of the very same kind, though differing of course in degree, as that which a scientific man goes through in tracing the causes of natural phenomena.

A very trivial circumstance will serve to exemplify this. Suppose you go into a fruiterer's shop, wanting an apple: you take up one, and, on biting it, you find it is sour; you look at it, and see that it is hard and green. You take up another one, and that too is hard, green, and sour. The shopman offers you a third; but, before biting it, you examine it, and find that it is hard and green, and you immediately say that you will not have it, as it must be sour, like those that you have already tried.

Nothing can be more simple than that, you think; but if you will take the trouble to analyze and trace out into its logical elements what has been done by the mind, you will be greatly surprised. In the first place, you have performed the operation of induction. You found that, in two experiences, hardness and greenness in apples go together with sourness. It was so in the first case, and it was confirmed by the second. True, it is a very small basis, but still it is enough to make an induction from; you generalize the facts, and you expect to find sourness in apples where you get hardness and greenness. You found upon that a general law, that all hard and green apples are sour; and that, so far as it goes, is a perfect induction. Well, having got your natural law in this way, when you are offered another apple which you find is hard and green, you say, "All hard and green apples are sour; this apple is hard and green, therefore this apple is sour." That train of reasoning is what logicians call a syllogism, and has all its various parts and terms—its major premiss, its minor premiss, and its conclusion. And, by the help of further reasoning, which, if drawn out, would have to be exhibited in two or three other syllogisms, you arrive at your final determination, "I will not have that apple." So that, you see, you have, in the first place, established a law by induction, and upon that you have founded a deduction, and reasoned out the special conclusion of the particular case. Well, now, suppose, having got your law, that at some time afterward, you are discussing the qualities of apples with a friend: you will say to him, "It is a very curious thing—but I find that all hard and green apples are sour!" Your friend says to you, "But how do you know that?" You at once reply, "Oh, because I have tried it over and over again, and have always found them to be so." Well, if we were talking science instead of common sense, we should call that an experimental verification. And, if still opposed, you go further, and say, "I have heard from the people in Somersetshire and Devonshire, where a large number of apples are grown, that they have observed the same thing. It is also found to be the case in Normandy and in North America. In short, I find it to be the universal experience of mankind wherever attention has been directed to the subject." Whereupon your friend, unless he is a very unreasonable man, agrees with you, and is convinced that you are quite right in the conclusion you have drawn. He believes, although perhaps he does not know he believes it, that the more extensive verifi-

cations are—that the more frequently experiments have been made, and results of the same kind arrived at—that the more varied the conditions under which the same results have been attained, the more certain is the ultimate conclusion, and he disputes the question no further. He sees that the experiment has been tried under all sorts of conditions, as to time, place, and people, with the same result; and he says with you, therefore, that the law you have laid down must be a good one, and he must believe it.

In science we do the same thing: the philosopher exercises precisely the same faculties, though in a much more delicate manner. In scientific inquiry it becomes a matter of duty to expose a supposed law to every possible kind of verification, and to take care, moreover, that this is done intentionally, and not left to a mere accident, as in the case of the apples. And in science, as in common life, our confidence in a law is in exact proportion to the absence of variation in the result of our experimental verifications. For instance, if you let go your grasp of an article you may have in your hand, it will immediately fall to the ground. That is a very common verification of one of the best established laws of nature—that of gravitation. The method by which men of science establish the existence of that law is exactly the same as that by which we have established the trivial proposition about the sourness of hard and green apples. But we believe it in such an extensive, thorough, and unhesitating manner because the universal experience of mankind verifies it, and we can verify it ourselves at any time; and that is the strongest possible foundation on which any natural law can rest.

So much by way of proof that the method of establishing laws in science is exactly the same as that pursued in common life. Let us now turn to another matter (though really it is but another phase of the same question), and that is, the method by which, from the relations of certain phenomena, we prove that some stand in the position of causes toward the others.

I want to put the case clearly before you, and I will therefore show you what I mean by another familiar example. I will suppose that one of you, on coming down in the morning to the parlor of your house, finds that a teapot and some spoons which had been left in the room on the previous evening are gone—the window is open, and you observe the mark of a dirty hand on the window-frame, and perhaps, in addition to that, you notice the impress of a hob-nailed shoe on the gravel outside. All these phenomena have struck your attention instantly, and before two minutes have passed you say, "Oh, somebody has broken open the window, entered the room, and run off with the spoons and the teapot!" That speech is out of your mouth in a moment. And you will probably add, "I know there has; I am quite sure of it!" You mean to say exactly what you know; but in reality what you have said has been the expression of what is, in all essential particulars, a hypothesis. You do not *know* it at all; it is nothing but a hypothesis rapidly framed in your own mind! And it is a hypothesis founded on a long train of inductions and deductions.

What are those inductions and deductions, and how have you got at this hypothesis? You have observed, in the first place, that the window is open; but by a train of reasoning involving many inductions and deductions, you have probably arrived long before at the general law—and a very good one it is—that windows do not open of themselves; and you therefore conclude that something has opened the window. A second general law that you have arrived at in the same way is, that teapots and spoons do not go out of a window spontaneously, and you are satisfied that, as they are not now where you left them, they have been removed. In the third place, you look at the marks on the window-sill, and the shoe-marks outside, and you say that in all previous experience the former kind of mark has never been produced by anything else but the hand of a human being; and the same experience shows that no other animal but man at present wears shoes with hob-nails on them, such as would produce the marks in the gravel. I do not know, even if we could discover any of those "missing links" that are talked about, that they would help us to any other conclusion! At any rate, the law which states our present experience is strong enough for my present purpose. You next reach the conclusion, that as these kinds of marks have not been left by any other animals than men, or are liable to be formed in any other way than by a man's hand and shoe, the marks in question have been formed by a man in that way. You have, further, a general law, founded on observation and experience—and that, too, is, I am sorry to say, a very universal and unimpeachable one—that some men are thieves; and you assume at once from all these premisses—and that is what constitutes your hypothesis—that the man who made the marks outside and on the window-sill, opened the window, got into the room, and stole your teapot and spoons. You have now arrived at a *vera causa*; you have assumed a cause which it is plain is competent to produce all the phenomena you have observed. You can explain all these phenomena have observed. You can explain all these only by the hypothesis of a thief. But that is a hypothetical conclusion, of the justice of which you have no absolute proof at all: it is only rendered highly probable by a series of inductive and deductive reasonings.

I suppose your first action, assuming you are a man of ordinary common-sense, and that you have established this hypothesis to your own satisfaction, will very likely be to go off for the police, and set them on the track of the burglar, with the view to the recovery of your property. But just as you are starting with this object, some person

comes in, and on learning what you are about, says, "My good friend, you are going on a great deal too fast. How do you know that the man who really made the marks took the spoons? It might have been a monkey that took them, and the man may have merely looked in afterward." You would probably reply, "Well, that is all very well, but you see it is contrary to all experience of the way teapots and spoons are abstracted; so that, at any rate, your hypothesis is less probable than mine." While you are talking the thing over in this way, another friend arrives, one of that good kind of people that I was talking of a little while ago. And he might say, "Oh, my dear sir, you are certainly going on a great deal too fast. You are most presumptuous. You admit that all these occurrences took place when you were fast asleep, at a time when you could not possibly have known anything about what was taking place. How do you know that the laws of nature are not suspended during the night? It may be that there has been some kind of supernatural interference in this case." In point of fact he declares that your hypothesis is one of which you cannot at all demonstrate the truth, and that you are by no means sure that the laws of nature are the same when you are asleep as when you are awake.

Well, now, you cannot at the moment answer that kind of reasoning. You feel that your worthy friend has you somewhat at a disadvantage. You will feel perfectly convinced in your own mind, however, that you are quite right, and you say to him, "My good friend, I can only be guided by the natural probabilities of the case, and if you will be kind enough to stand aside and permit me to pass, I will go and fetch the police." Well, we will suppose that your journey is successful, and that by good luck you meet with a policeman; that eventually the burglar is found with your property on his person, and the marks correspond to his hand and to his boots. Probably any jury would consider those facts a very good experimental verification of your hypothesis, touching the cause of the abnormal phenomena observed in your parlor, and would act accordingly.

Now, in this supposititious case I have taken phenomena of a very common kind, in order that you might see what are the different steps in an ordinary process of reasoning, if you will only take the trouble to analyze it carefully. All the operations I have described, you will see, are involved in the mind of any man of sense in leading him to a conclusion as to the course he should take in order to make good a robbery and punish the offender. I say that you are led, in that case, to your conclusion by exactly the same train of reasoning as that which a man of science pursues when he is endeavoring to discover the origin and laws of the most occult phenomena. The process is, and always must be, the same; and precisely the same mode of reasoning was employed by Newton and Laplace in their endeavors to discover and define the causes of the movements of the heavenly bodies, as you, with your own common-sense, would employ to detect a burglar. The only difference is, that the nature of the inquiry being more abstruse, every step has to be most carefully watched, so that there may not be a single crack or flaw in your hypothesis. A flaw or crack in many of the hypotheses of daily life may be of little or no moment as affecting the general correctness of the conclusions at which we may arrive; but in a scientific inquiry a fallacy, great or small, is always of importance, and is sure to be constantly productive of mischievous, if not fatal, results in the long run.

Do not allow yourselves to be misled by the common notion that a hypothesis is untrustworthy simply because it is a hypothesis. It is often urged, in respect to some scientific conclusion, that, after all, it is only a hypothesis. But what more have we to guide us in nine tenths of the most important affairs of daily life than hypotheses, and often very ill-based ones? So that in science, where the evidence of a hypothesis is subjected to the most rigid examination, we may rightly pursue the same course. You may have hypotheses and hypotheses. A man may say, if he likes, that the moon is made of green cheese: that is a hypothesis. But another man, who has devoted a great deal of time and attention to the subject, and availed himself of the most powerful telescopes and the results of the observations of others, declares that in his opinion it is probably composed of materials very similar to those of which our own earth is made up: and that is also only a hypothesis. But I need not tell you that there is an enormous difference in the value of the two hypotheses. That one which is based on sound scientific knowledge is sure to have a corresponding value; and that which is a mere hasty, random guess is likely to have but little value. Every great step in our progress in discovering causes has been made in exactly the same way as that which I have detailed to you. A person observing the occurrence of certain facts and phenomena asks, naturally enough, what process, what kind of operation known to occur in nature applied to the particular case, will unravel and explain the mystery? Hence you have the scientific hypothesis; and its value will be proportionate to the care and completeness with which its basis had been tested and verified. It is in these matters as in the commonest affairs of practical life: the guess of the fool will be folly, while the guess of the wise man will contain wisdom. In all cases, you see that the value of the result depends on the patience and faithfulness with which the investigator applies to his hypothesis every possible kind of verification.

I dare say I may have to return to this point by and by; but having dealt thus far with our logical methods, I must now turn to something which, perhaps, you may con-

sider more interesting, or, at any rate, more tangible. But in reality there are but few things that can be more important for you to understand than the mental processes and the means by which we obtain scientific conclusions and theories.* Having granted that the inquiry is a proper one, and having determined on the nature of the methods we are to pursue, and which only can lead to success, I must now turn to the consideration of our knowledge of the nature of the processes which have resulted in the present condition of organic nature.

Here, let me say at once, lest some of you misunderstand me, that I have extremely little to report. The question of how the present condition of organic nature came about resolves itself into two questions. The first is, How has organic or living matter commenced its existence? And the second is, How has it been perpetuated? On the second question I shall have more to say hereafter. But on the first one, what I now have to say will be for the most part of a negative character.

If you consider what kind of evidence we can have upon this matter, it will resolve itself into two kinds. We may have historical evidence and we may have experimental evidence. It is, for example, conceivable, that inasmuch as the hardened mud which forms a considerable portion of the thickness of the earth's crust contains faithful records of the past forms of life, and inasmuch as these differ more and more as we go further down—it is possible and conceivable that we might come to some particular bed or stratum which should contain the remains of those creatures with which organic life began upon the earth. And if we did so, and if such forms of organic life were preservable, we should have what I would call historical evidence of the mode in which organic life began upon this planet. Many persons will tell you, and indeed you will find it stated in many works on geology, that this has been done, and that we really possess such a record; there are some who imagine that the earliest forms of life of which we have as yet discovered any records are in truth the forms in which animal life began upon the globe. The grounds on which they base that supposition are these: That if you go through the enormous thickness of the earth's crust and get down to the older rocks, the higher vertebrate animals--the quadrupeds, birds, and fishes—cease to be found; beneath them you find only the invertebrate animals; and in the deepest and lowest rocks those remains become scantier and scantier, not in any very gradual progression, however, until, at length, in what are supposed to be the oldest rocks, the animal remains which are found are almost always confined to four forms—*Oldhamia*, whose precise nature is not known, whether plant or animal; *Lingula*, a kind of mollusk; *Trilobites*, a crustacean animal, having the same essential plan of construction, though differing in many details from a lobster or crab; and *Hymenocaris*, which is also a crustacean. So that you have all the *Fauna* reduced, at this period, to four forms: one a kind of animal or plant that we know nothing about, and three undoubted animals—two crustaceans and one mollusk.

I think, considering the organization of these mollusca and crustacea, and looking at their very complex nature, that it does indeed require a very strong imagination to conceive that these were the first created of all living things. And you must take into consideration the fact that we have not the slightest proof that these which we call the oldest beds are really so: I repeat, we have not the slightest proof of it. When you find in some places that in an enormous thickness of rocks there are but very scanty traces of life, or absolutely none at all; and that in other parts of the world rocks of the very same formation are crowded with the records of living forms; I think it is impossible to place any reliance on the supposition, or to feel one's self justified in supposing that these are the forms in which life first commenced. I have not time here to enter upon the technical grounds upon which I am led to this conclusion—that could hardly be done properly in half a dozen lectures on that part alone; I must content myself with saying that I do not at all believe that these are the oldest forms of life.

I turn to the experimental side to see what evidence we have there. To enable us to say that we know anything about the experimental origination of organization and life, the investigator ought to be able to take inorganic matters, such as carbonic acid, ammonia, water, and salines, in any sort of inorganic combination, and be able to build them up into proteine matter, and that that proteine matter ought to begin to live in an organic form. That, nobody has done as yet, and I suspect it will be a long while before anybody does do it. But the thing is by no means so impossible as it looks; for the researches of modern chemistry have shown us—I won't say the road toward it, but, if I may so say, they have shown the finger-post pointing to the road that may lead to it.

It is not many years ago—and you must recollect that organic chemistry is a young science, not above a couple of generations old—you must not expect too much of it; it is not many years ago since it was said to be perfectly impossible to fabricate any organic compound; that is to say, any non-mineral compound which is to be found in an organized being. It remained so for a very long period; but it is now a considerable number of years since a distinguished foreign chemist contrived to fabricate urea, a substance of a very complex character which forms one of the waste products of animal structures. And of late years a number of other compounds, such as butyric acid, and others, have been added to the list. I need not tell

* Those who wish to study fully the doctrines of which I have endeavored to give some rough and ready illustrations, must read Mr. John Stuart Mill's "System of Logic."

you that chemistry is an enormous distance from the goal I indicate; all I wish to point out to you is, that it is by no means safe to say that that goal may not be reached one day. It may be that it is impossible for us to produce the conditions requisite to the origination of life; but we must speak modestly about the matter, and recollect that Science has put her foot upon the bottom round of the ladder. Truly he would be a bold man who would venture to predict where she will be fifty years hence.

There is another inquiry which bears indirectly upon this question, and upon which I must say a few words. You are all of you aware of the phenomena of what is called spontaneous generation. Our forefathers, down to the seventeenth century or thereabouts, all imagined, in perfectly good faith, that certain vegetable and animal forms gave birth, in the process of their decomposition, to insect life. Thus, if you put a piece of meat in the sun, and allowed it to putrefy, they conceived that the grubs which soon began to appear were the result of the action of a power of spontaneous generation which the meat contained. And they could give you receipts for making various animal and vegetable preparations which would produce particular kinds of animals. A very distinguished Italian naturalist, named Redi, took up the question at a time when everybody believed in it; among others our own great Harvey, the discoverer of the circulation of the blood. You will constantly find his name quoted, however, as an opponent of the doctrine of spontaneous generation; but the fact is, and you will see it if you will take the trouble to look into his works, Harvey believed it is as profoundly as any man of his time; but he happened to enunciate a very curious proposition—that every living thing came from an *egg*; he did not mean to use the word in the sense in which we now employ it, he only meant to say that every living thing originated in a little rounded particle of organized substance; and it is from this circumstance, probably, that the notion of Harvey having opposed the doctrine originated. Then came Redi, and he proceeded to upset the doctrine in a very simple manner. He merely covered the piece of meat with some very fine gauze, and then he exposed it to the same conditions. The result of this was that no grubs or insects were produced; he proved that the grubs originated from the insects who came and deposited their eggs in the meat, and that they were hatched by the heat of the sun. By this kind of inquiry he thoroughly upset the doctrine of spontaneous generation, for his time at least.

Then came the discovery and application of the microscope to scientific inquiries, which showed to naturalists that besides the organisms which they already knew as living beings and plants, there were an immense number of minute things which could be obtained apparently almost at will from decaying vegetable and animal forms. Thus, if you took some ordinary black pepper or some hay, and steeped it in water, you would find in the course of a few days that the water had become impregnated with an immense number of animalcules swimming about in all directions. From facts of this kind naturalists were led to revive the theory of spontaneous generation. They were headed here by an English naturalist—Needham—and afterward in France by the learned Buffon. They said that these things were absolutely begotten in the water of the decaying substances out of which the infusion was made. It did not matter whether you took animal or vegetable matter, you had only to steep it in water and expose it, and you would soon have plenty of animalcules. They made a hypothesis about this which was a very fair one. They said, this matter of the animal world, or of the higher plants, appears to be dead, but in reality it has a sort of dim life about it, which, if it is placed under fair conditions, will cause it to break up into the forms of these little animalcules, and they will go through their lives in the same way as the animal or plant of which they once formed a part.

The question now became very hotly debated. Spallanzani, an Italian naturalist, took up opposite views to those of Needham and Buffon, and by means of certain experiments he showed that it was quite possible to stop the process by boiling the water, and closing the vessel in which it was contained. "Oh!" said his opponents; "but what do you know you may be doing when you heat the air over the water in this way? You may be destroying some property of the air requisite for the spontaneous generation of the animalcules."

However, Spallanzani's views were supposed to be upon the right side, and those of the others fell into discredit; although the fact was that Spallanzani had not made good his views. Well, then, the subject continued to be revived from time to time, and experiments were made by several persons; but these experiments were not altogether satisfactory. It was found that if you put an infusion in which animalcules would appear if it were exposed to the air into a vessel and boiled it, and then sealed up the mouth of the vessel, so that no air, save such as had been heated to 212°, could reach its contents, that then no animalcules would be found; but if you took the same vessel and exposed the infusion to the air, then you would get animalcules. Furthermore, it was found that if you connected the mouth of the vessel with a red-hot tube in such a way that the air would have to pass through the tube before reaching the infusion, that then you would get no animalcules. Yet another thing was noticed: if you took two flasks containing the same kind of infusion, and left one entirely exposed to the air, and in the mouth of the other placed a ball of cotton-wool, so that the air would have to filter itself through it before reaching the infusion, that then, although you might have plenty

of animalcules in the first flask, you would certainly obtain none from the second.

These experiments, you see, all tended toward one conclusion—that the infusoria were developed from little minute spores or eggs which were constantly floating in the atmosphere, which lose their power of germination if subjected to heat. But one observer now made another experiment, which seemed to go entirely the other way, and puzzled him altogether. He took some of this boiled infusion that I have been speaking of, and by the use of a mercurial bath—a kind of trough used in laboratories—he deftly inverted a vessel containing the infusion into the mercury, so that the latter reached a little beyond the level of the mouth of the *inverted* vessel. You see that he thus had a quantity of the infusion shut off from any possible communication with the outer air by being inverted upon a bed of mercury.

He then prepared some pure oxygen and nitrogen gases, and passed them by means of a tube going from the outside of the vessel, up through the mercury into the infusion; so that he thus had it exposed to a perfectly pure atmosphere of the same constituents as the external air. Of course, he expected he would get no infusorial animalcules at all in that infusion; but, to his great dismay and discomfiture, he found he almost always did get them.

Furthermore it has been found that experiments made in the manner described above answer well with most infusions; but that if you fill the vessel with boiled milk and then stop the neck with cotton-wool you *will* have infusoria. So that you see there were two experiments that brought you to one kind of conclusion, and three to another; which was a most unsatisfactory state of things to arrive at in a scientific inquiry.

Some few years after this, the question began to be very hotly discussed in France. There was M. Pouchet, a professor at Rouen, a very learned man, but certainly not a very rigid experimentalist. He published a number of experiments of his own, some of which were very ingenious, to show that if you went to work in a proper way, there was a truth in the doctrine of spontaneous generation. Well, it was one of the most fortunate things in the world that M. Pouchet took up this question, because it induced a distinguished French chemist, M. Pasteur, to take up the question on the other side; and he has certainly worked it out in the most perfect manner. I am glad to say, too, that he has published his researches in time to enable me to give you an account of them. He verified all the experiments which I have just mentioned to you—and then finding those extraordinary anomalies, as in the case of the mercury bath and the milk, he set himself to work to discover their nature. In the case of milk he found it to be a question of temperature. Milk in a fresh state is slightly alkaline; and it is a very curious circumstance, but this very slight degree of alkalinity seems to have the effect of preserving the organisms which fall into it from the air from being destroyed at a temperature of 212°, which is the boiling point. But if you raise the temperature 10° when you boil it, the milk behaves like everything else; and if the air with which it comes in contact, after being boiled at this temperature, is passed through a red-hot tube, you will not get a trace of organisms.

He then turned his attention to the mercury bath, and found on examination that the surface of the mercury was almost always covered with a very fine dust. He found that even the mercury itself was positively full of organic matters; that from being constantly exposed to the air, it had collected an immense number of these infusorial organisms from the air. Well, under these circumstances he felt that the case was quite clear, and that the mercury was not what it had appeared to M. Schwann to be—a bar to the admission of these organisms; but that in reality, it acted as a reservoir from which the infusion was immediately supplied with the large quantity that had so puzzled him.

But not content with explaining the experiments of others, M. Pasteur went to work to satisfy himself completely. He said to himself: "If my view is right, and if, in point of fact, all these appearances of spontaneous generation are altogether due to the falling of minute germs suspended in the atmosphere, why, I ought not only to be able to show the germs, but I ought to be able to catch and sow them, and produce the resulting organisms." He, accordingly, constructed a very ingenious apparatus to enable him to accomplish this trapping of this "*germ dust*" in the air. He fixed in the window of his room a glass tube, in the centre of which he had placed a ball of gun-cotton, which, as you all know, is ordinary cotton-wool, which, from having been steeped in strong acid, is converted into a substance of great explosive power. It is also soluble in alcohol and ether. One end of the glass tube was, of course, open to the external air; and at the other end of it he placed an aspirator, a contrivance for causing a current of the external air to pass through the tube. He kept his apparatus going for four-and-twenty hours, and then removed the *dusted* gun-cotton, and dissolved it in alcohol and ether. He then allowed this to stand for a few hours, and the result was, that a very fine dust was gradually deposited at the bottom of it. That dust, on being transferred to the stage of a microscope, was found to contain an enormous number of starch grains. You know that the materials of our food and the greater portion of plants are composed of starch, and we are constantly making use of it in a variety of ways, so that there is always a quantity of it suspended in the air. It is these starch grains which form many of those bright specks that we see dancing in a ray of light sometimes. But besides these, M. Pasteur found also an immense number of other organic

substances, such as spores of fungi, which had been floating about in the air and had got caged in this way.

He went farther, and said to himself, "If these really are the things that give rise to the appearance of spontaneous generation, I ought to be able to take a ball of this *dusted* gun-cotton and put it into one of my vessels, containing that boiled infusion which has been kept away from the air, and in which no infusoria are at present developed, and then, if I am right, the introduction of this gun-cotton will give rise to organisms."

Accordingly, he took one of these vessels of infusion, which had been kept eighteen months, without the least appearance of life, and by a most ingenious contrivance he managed to break it open and introduce such a ball of gun-cotton, without allowing the infusion or the cotton ball to come into contact with any air but that which had been subjected to a red heat, and in twenty-four hours he had the satisfaction of finding all the indications of what had been hitherto called spontaneous generation. He had succeeded in catching the germs and developing organisms in the way he had anticipated.

It now struck him that the truth of his conclusions might be demonstrated without all the apparatus he had employed. To do this, he took some decaying animal or vegetable substance, such as urine, which is an extremely decomposable substance, or the juice of yeast, or perhaps some other artificial preparation, and filled a vessel having a long tubular neck, with it. He then boiled the liquid and bent that long neck into an S shape or zig-zag, leaving it open at the end. The infusion then gave no trace of any appearance of spontaneous generation, however long it might be left, as all the germs in the air were deposited in the beginning of the bent neck. He then cut the tube close to the vessel, and allowed the ordinary air to have free and direct access; and the result of that was the appearance of organisms in it, as soon as the infusion had been allowed to stand long enough to allow of the growth of those it received from the air, which was about forty-eight hours. The result of M. Pasteur's experiments proved, therefore, in the most conclusive manner, that all the appearances of spontaneous generation arose from nothing more than the deposition of the germs of organisms which were constantly floating in the air.

To this conclusion, however, the objection was made, that if that were the cause, then the air would contain such an enormous number of these germs, that it would be a continual fog. But M. Pasteur replied that they are not there in anything like the number we might suppose, and that an exaggerated view has been held on that subject; he showed that the chances of animal or vegetable life appearing in infusions depend entirely on the conditions under which they are exposed. If they are exposed to the ordinary atmosphere around us, why, of course, you may have organisms appearing early. But, on the other hand, if they are exposed to air from a great height, or from some very quiet cellar, you will often not find a single trace of life.

So that M. Pasteur arrived at last at the clear and definite result, that all these appearances are like the case of the worms in the piece of meat, which was refuted by Redi, simply germs carried by the air and deposited in the liquids in which they afterward appear. For my own part, I conceive that, with the particulars of M. Pasteur's experiments before us, we cannot fail to arrive at his conclusions, and that the doctrine of spontaneous generation has received a final *coup de grace*.

You, of course, understand that all this in no way interferes with the *possibility* of the fabrication of organic matters by the direct method to which I have referred, remote as that possibility may be.

LECTURE IV.

THE PERPETUATION OF LIVING BEINGS, HEREDITARY TRANSMISSION AND VARIATION.

The inquiry which we undertook, at our last meeting, into the state of our knowledge of the causes of the phenomena of organic nature—of the past and of the present—resolved itself into two subsidiary inquiries: the first was, whether we know anything, either historically or experimentally, of the mode of origin of living beings; the second subsidiary inquiry was, whether, granting the origin, we know anything about the perpetuation and modifications of the forms of organic beings. The reply which I had to give to the first question was altogether negative, and the chief result of my last lecture was, that neither historically nor experimentally do we at present know anything whatsoever about the origin of living forms. We saw that historically we are not likely to know anything about it, although we may perhaps learn something experimentally, but that at present we are an enormous distance from the goal I indicated.

I now, then, take up the next question, What do we know of the reproduction, the perpetuation, and the modifications of the forms of living beings, supposing that we have put the question as to their origination on one side, and have assumed that at present the causes of their origination are beyond us, and that we know nothing about them? Upon this question the state of our knowledge is extremely different; it is exceedingly large, and, if not complete, our experience is certainly most extensive. It would be impossible to lay it all before you, and the most I can do, or need do to-night, is to take up the principal points and put them before you with such prominence as may subserve the purposes of our present argument.

The method of the perpetuation of organic beings is of two kinds—the asexual and the sexual. In the first the perpetuation takes place from and by a particular act of an indi-

vidual organism, which sometimes may not be classed as belonging to any sex at all. In the second case, it is in consequence of the mutual action and interaction of certain portions of the organisms of usually two distinct individuals—the male and the female. The cases of asexual perpetuation are by no means so common as the cases of sexual perpetuation; and they are by no means so common in the animal as in the vegetable world. You are all probably familiar with the fact, as a matter of experience, that you can propagate plants by means of what are called "cuttings;" for example, that by taking a cutting from a geranium plant, and rearing it properly, by supplying it with light and warmth and nourishment from the earth, it grows up and takes the form of its parent, having all the properties and peculiarities of the original plant.

Sometimes this process, which the gardener performs artificially, takes place naturally; that is to say, a little bulb, or portion of the plant, detaches itself, drops off, and becomes capable of growing as a separate thing. That is the case with many bulbous plants, which throw off in this way secondary bulbs, which are lodged in the ground and become developed into plants. This is an asexual process, and from it results the repetition or reproduction of the form of the original being from which the bulb proceeds.

Among animals the same thing takes place. Among the lower forms of animal life, the infusorial animalculæ we have already spoken of throw off certain portions, or break themselves up in various directions, sometimes transversely or sometimes longitudinally; or they may give off buds, which detach themselves and develop into their proper forms. There is the common fresh-water polype, for instance, which multiplies itself in this way. Just in the same way as the gardener is able to multiply and reproduce the peculiarities and characters of particular plants by means of cuttings, so can the physiological experimentalist—as was shown by the Abbé Trembley many years ago—so can he do the same thing with many of the lower forms of animal life. M. de Trembley showed that you could take a polype and cut it into two, or four, or many pieces, mutilating it in all directions, and the pieces would still grow up and reproduce completely the original form of the animal. These are all cases of asexual multiplication, and there are other instances, and still more extraordinary ones, in which this process takes place naturally, in a more hidden, a more recondite kind of way. You are all of you familiar with those little green insects, the *Aphis*, or blight, as it is called. These little animals, during a very considerable part of their existence, multiply themselves by means of a kind of internal budding, the buds being developed into essentially asexual animals, which are neither male nor female; they become converted into young *Aphides*, which repeat the process, and their offspring after them, and soon again; you may go on for nine or ten, or even twenty or more successions; and there is no very good reason to say how soon it might terminate, or how long it might not go on if the proper conditions of warmth and nourishment were kept up.

Sexual reproduction is quite a distinct matter. Here, in all these cases, what is required is the detachment of two portions of the parental organisms, which portions we know as the egg or the spermatozoon. In plants it is the ovule and the pollen-grain, as in the flowering plants, or the ovule and the antherozooid, as in the flowerless. Among all forms of animal life, the spermatozoa proceed from the male sex, and the egg is the product of the female. Now, what is remarkable about this mode of reproduction is this, that the egg by itself, or the spermatozoa by themselves, are unable to assume the parental form; but if they be brought into contact with one another, the effect of the mixture of organic substances proceeding from two sources appears to confer an altogether new vigor to the mixed product. This process is brought about, as we all know, by the sexual intercourse of the two sexes, and is called the act of impregnation. The result of this act on the part of the male and female is, that the formation of a new being is set up in the ovule or egg; this ovule or egg soon begins to be divided and subdivided, and to be fashioned into various complex organisms, and eventually to develop into the form of one of its parents, as I explained in the first lecture. These are the processes by which the perpetuation of organic beings is secured. Why there should be the two modes—why this reinvigoration should be required on the part of the female element, we do not know; but it is most assuredly the fact, and it is presumable, that, however long the process of asexual multiplication could be continued—I say there is good reason to believe that it would come to an end if a new commencement were not obtained by a conjunction of the two sexual elements.

That character which is common to these two distinct processes is this, that, whether we consider the reproduction, or perpetuation, or modification of organic beings as they take place asexually, or as they may take place sexually—in either case, I say, the offspring, has a constant tendency to assume, speaking generally, the character of the parent. As I said just now, if you take a slip of a plant, and tend it with care, it will eventually grow up and develop into a plant like that from which it had sprung; and this tendency is so strong that, as gardeners know, this mode of multiplying by means of cuttings is the only secure mode of propagating very many varieties of plants; the peculiarity of the primitive stock seems to be better preserved if you propagate it by means of a slip than if you resort to the sexual mode.

Again, in experiments upon the lower animals, such as the polype, to which I have

referred, it is most extraordinary that, although cut up into various pieces, each particular piece will grow up into the form of the primitive stock; the head, if separated, will reproduce the body and the tail; and if you cut off the tail you will find that that will reproduce the body and all the rest of the members, without in any way deviating from the plan of the organism from which these portions have been detached. And so far does this go, that some experimentalists have carefully examined the lower orders of animals—among them the Abbé Spallanzani, who made a number of experiments upon snails and salamanders—and have found that they might mutilate them to an incredible extent; that you might cut off the jaw or the greater part of the head, or the leg or the tail, and repeat the experiment several times, perhaps cutting off the same member again and again; and yet each of those types would be reproduced according to the primitive type; nature making no mistake, never putting on a fresh kind of leg, or head, or tail, but always tending to repeat and to return to the primitive type.

It is the same in sexual reproduction: it is a matter of perfectly common experience, that the tendency on the part of the offspring always is, speaking broadly, to reproduce the form of the parents. The proverb has it that the thistle does not bring forth grapes; so, among ourselves, there is always a likeness, more or less marked and distinct, between children and their parents. That is a matter of familiar and ordinary observation. We notice the same thing occurring in the cases of the domestic animals—dogs, for instance, and their offspring. In all these cases of propagation and perpetuation there seems to be a tendency in the offspring to take the characters of the parental organisms. To that tendency a special name is given—and as I may very often use it I will write it up here on this blackboard that you may remember it—it is called *Atavism*; it expresses this tendency to revert to the ancestral type, and comes from the Latin word *atavus*, ancestor.

Well, this *Atavism* which I shall speak of, is, as I said before, one of the most marked and striking tendencies of organic beings; but, side by side with this hereditary tendency, there is an equally distinct and remarkable tendency to variation. The tendency to reproduce the original stock has, as it were, its limits, and side by side with it there is a tendency to vary in certain directions, as if there were two opposing powers working upon the organic being, one tending to take it in a straight line, and the other tending to make it diverge from that straight line, first to one side and then to the other.

So that you see these two tendencies need not precisely contradict one another, as the ultimate result may not always be very remote from what would have been the case if the line had been quite straight.

This tendency to variation is less marked in that mode of propagation which takes place asexually; it is in that mode that the minor characters of animal and vegetable structures are most completely preserved. Still, it will happen sometimes, that the gardener, when he has planted a cutting of some favorite plant, will find, contrary to his expectation, that the slip grows up a little different from the primitive stock—that it produces flowers of a different color or make, or some deviation in one way or another. This is what is called the "sporting" of plants.

In animals the phenomena of asexual propagation are so obscure that at present we cannot be said to know much about them; but if we turn to that mode of perpetuation which results from the sexual process, then we find variation a perfectly constant occurrence, to a certain extent; and, indeed, I think that a certain amount of variation from the primitive stock is the necessary result of the method of sexual propagation itself; for, inasmuch as the thing propagated proceeds from two organisms of different sexes and different makes and temperaments, and as the offspring is to be either of one sex or the other, it is quite clear that it cannot be an exact diagonal of the two, or it would be of no sex at all; it cannot be an exact intermediate form between that of each of its parents—it must deviate to one side or the other. You do not find that the male follows the precise type of the male parent, nor does the female always inherit the precise characteristics of the mother—there is always a proportion of the female character in the male offspring, and of the male character in the female offspring. That must be quite plain to all of you who have looked at all attentively on your own children or those of your neighbors; you will have noticed how very often it may happen that the son shall exhibit the maternal type of character, or the daughter possess the characteristics of the father's family. There are all sorts of intermixtures and intermediate conditions between the two, where complexion, or beauty, or fifty other different peculiarities belonging to either side of the house are reproduced in other members of the same family. Indeed, it is sometimes to be remarked in this kind of variation, that the variety belongs, strictly speaking, to neither of the immediate parents; you will see a child in a family who is not like either its father or its mother; but some old person who knew its grandfather or grandmother, or, it may be, an uncle, or, perhaps, even a more distant relative, will see a great similarity between the child and one of these. In this way it constantly happens that the characteristic of some previous member of the family comes out and is reproduced and recognized in the most unexpected manner.

But apart from that matter of general experience, there are some cases which put that curious mixture in a very clear light. You are aware that the offspring of the ass and the horse, or rather of the he-ass and the mare, is what is called a mule; and, on the

other hand, the offspring of the stallion and the she-ass is what is called a *hinny*. It is a very rare thing in this country to see a hinny. I never saw one myself; but they have been very carefully studied. Now, the curious thing is this, that although you have the same elements in the experiment in each case the offspring is entirely different in character, according as the male influence comes from the ass or the horse. Where the ass is the male, as in the case of the mule, you find that the head is like that of the ass, that the ears are long, the tail is tufted at the end, the feet are small, and the voice is an unmistakable bray; these are all points of similarity to the ass; but, on the other hand, the barrel of the body and the cut or the neck are much more like those of the mare. Then, if you look at the hinny—the result of the union of the stallion and the she-ass, then you find it is the horse that has the predominance; that the head is more like that of the horse, the ears are shorter, the legs coarser, and the type is altogether altered; while the voice, instead of being a bray, is the ordinary neigh of the horse. Here, you see, is a most curious thing: you take exactly the same elements, ass and horse, but you combine the sexes in a different manner, and the result is modified accordingly. You have in this case, however, a result which is not general and universal—there is usually an important preponderance, but not always on the same side.

Here, then, is one intelligible, and, perhaps, necessary cause of variation: the fact, that there are two sexes sharing in the production of the offspring, and that the share taken by each is different and variable, not only for each combination, but also for different members of the same family.

Secondly, there is a variation, to a certain extent—though in all probability the influence of this cause has been very much exaggerated—but there is no doubt that variation is produced to a certain extent by what are commonly known as external conditions—such as temperature, food, warmth, and moisture. In the long run, every variation depends, in some sense, upon external conditions, seeing that everything has a cause of its own. I use the term "external conditions" now in the sense in which it is ordinarily employed: certain it is, that external conditions have a definite effect. You may take a plant which has single flowers, and by dealing with the soil, and nourishment, and so on, you may by and by convert single flowers into double flowers, and make thorns shoot out into branches. You may thicken or make various modifications in the shape of the fruit. In animals, too, you may produce analogous changes in this way, as in the case of that deep bronze color which persons rarely lose after having passed any length of time in tropical countries. You may also alter the development of the muscles very much, by dint of training; all the world knows that exercise has a great effect in this way; we always expect to find the arm of a blacksmith hard and wiry, and possessing a large development of the brachial muscles. No doubt, training, which is one of the forms of external conditions, converts what are originally only instructions, teachings, into habits, or, in other words, into organizations, to a great extent; but this second cause of variation cannot be considered to be by any means a large one. The third cause that I have to mention, however, is a very extensive one. It is one that, for want of a better name, has been called "spontaneous variation;" which means that when we do not know anything about the cause of phenomena, we call it spontaneous. In the orderly chain of causes and effects in this world, there are very few things of which it can be said with truth that they are spontaneous. Certainly not in these physical matters—in these there is nothing of the kind—everything depends on previous conditions. But when we cannot trace the cause of phenomena, we call them spontaneous.

Of these variations, multitudinous as they are, but little is known with perfect accuracy. I will mention to you some two or three cases, because they are very remarkable in themselves, and also because I shall want to use them afterward. Réaumur, a famous French naturalist, a great many years ago, in the essay which he wrote upon the art of hatching chickens—which was indeed a very curious essay—had occasion to speak of variations and monstrosities. One very remarkable case had come under his notice of a variation in the form of a human member, in the person of a Maltese, of the name of Gratio Kelleia, who was born with six fingers upon each hand, and the like number of toes to each of his feet. That was a case of spontaneous variation. Nobody knows why he was born with that number of fingers and toes, and as we don't know, we call it a case of "spontaneous" variation. There is another remarkable case also. I select these, because they happen to have been observed and noted very carefully at the time. It frequently happens that a variation occurs, but the persons who notice it do not take any care in noting down the particulars until at length, when inquiries come to be made, the exact circumstances are forgotten; and hence, multitudinous as may be such "spontaneous" variations, it is exceedingly difficult to get at the origin of them.

The second case is one of which you may find the whole details in the "Philosophical Transactions" for the year 1813, in a paper communicated by Colonel Humphrey to the President of the Royal Society—"On a new Variety in the Breed of Sheep," giving an account of a very remarkable breed of sheep, which at one time was well known in the northern States of America, and which went by the name of the Ancon or the Otter breed of sheep. In the year 1791 there was a farmer of the name of Seth Wright, in Massachusetts, who had a flock of sheep, consisting of a ram and, I think, of some twelve or thirteen ewes. Of this flock of ewes, one

at the breeding-time bore a lamb which was very singularly formed; it had a very long body, very short legs, and those legs were bowed! I will tell you by and by how this singular variation in the breed of sheep came to be noted, and to have the prominence that it now has. For the present, I mention only these two cases; but the extent of variation in the breed of animals is perfectly obvious to any one who has studied natural history with ordinary attention, or to any person who compares animals with others of the same kind. It is strictly true that there are never any two specimens which are exactly alike; however similar, they will always differ in some certain particular.

Now let us go back to Atavism—to the hereditary tendency I spoke of. What will come of a variation when you breed from it, when Atavism comes, if I may say so, to intersect variation? The two cases of which I have mentioned the history give a most excellent illustration of what occurs. Gratio Kelleia, the Maltese, married when he was twenty-two years of age, and, as I suppose there were no six-fingered ladies in Malta, he married an ordinary five-fingered person. The result of that marriage was four children; the first, who was christened Salvator, had six fingers and six toes, like his father; the second was George, who had five fingers and toes, but one of them was deformed, showing a tendency to variation; the third was André; he had five fingers and five toes, quite perfect; the fourth was a girl, Marie; she had five fingers and five toes, but her thumbs were deformed, showing a tendency toward the sixth.

These children grew up, and when they came to adult years they all married, and of course it happened that they all married five-fingered and five-toed persons. Now let us see what were the results. Salvator had four children; they were two boys, a girl, and another boy: the first two boys and the girl were six-fingered and six-toed like their grandfather; the fourth boy had only five fingers and five toes. George had only four children: there were two girls with six fingers and six toes; there was one girl with six fingers and five toes on the right side, and five fingers and five toes on the left side, so that she was half and half. The last, a boy, had five fingers and five toes. The third, André, you will recollect, was perfectly well-formed, and he had many children, whose hands and feet were all regularly developed. Marie, the last, who, of course, married a man who had only five fingers, had four children: the first, a boy, was born with six toes, but the other three were normal.

Now observe what very extraordinary phenomena are presented here. You have an accidental variation arising from what you may call a monstrosity; you have that monstrosity tendency or variation diluted in the first instance by an admixture with a female of normal construction, and you would naturally expect that, in the results of such an union, the monstrosity, if repeated, would be in equal proportion with the normal type; that is to say, that the children would be half and half, some taking the peculiarity of the father, and the others being of the purely normal type of the mother; but you see we have a great preponderance of the abnormal type. Well, this comes to be mixed once more with the pure, the normal type, and the abnormal is again produced in large proportion, notwithstanding the second dilution. Now what would have happened if these abnormal types had intermarried with each other; that is to say, suppose the two boys of Salvator had taken it into their heads to marry their first cousins, the two first girls of George, their uncle? You will remember that these are all of the abnormal type of their grandfather. The result would probably have been, that their offspring would have been in every case a further development of that abnormal type. You see it is only in the fourth, in the person of Marie, that the tendency, when it appears but slightly in the second generation, is washed out in the third, while the progeny of André, who escaped in the first instance, escape altogether.

We have in this case a good example of nature's tendency to the perpetuation of a variation. Here it is certainly a variation which carried with it no use or benefit; and yet you see the tendency to perpetuation may be so strong that, notwithstanding a great admixture of pure blood, the variety continues itself up to the third generation, which is largely marked with it. In this case, as I have said, there was no means of the second generation intermarrying with any but five-fingered persons, and the question naturally suggests itself, What would have been the result of such marriage? Réaumur narrates this case only as far as the third generation. Certainly it would have been an exceedingly curious thing if we could have traced this matter any farther; had the cousins intermarried, a six-fingered variety of the human race might have been set up.

To show you that this supposition is by no means an unreasonable one, let me now point out what took place in the case of Seth Wright's sheep, where it happened to be a matter of moment to him to obtain a breed or raise a flock of sheep like that accidental variety that I have described—and I will tell you why. In that part of Massachusetts where Seth Wright was living the fields were separated by fences, and the sheep, which were very active and robust, would roam abroad, and without much difficulty jump over these fences into other people's farms. As a matter of course this exuberant activity on the part of the sheep constantly gave rise to all sorts of quarrels, bickerings, and contentions among the farmers of the neighborhood; so it occurred to Seth Wright, who was, like his successors, more or less 'cute, that if he could get a stock of sheep like those with the bandy legs, they would not be able to jump over the fences so readily, and he acted upon that idea. He

killed his old ram, and as soon as the young one arrived at maturity he bred altogether from it. The result was even more striking than in the human experiment which I mentioned just now. Colonel Humphreys testifies that it always happened that the offspring were either pure Ancons or pure ordinary sheep; that in no case was there any mixing of the Ancons with the others. In consequence of this, in the course of a very few years, the farmer was able to get a very considerable flock of this variety, and a large number of them were spread throughout Massachusetts. Most unfortunately, however—I suppose it was because they were so common—nobody took enough notice of them to preserve their skeletons; and although Colonel Humphreys states that he sent a skeleton to the president of the Royal Society at the same time that he forwarded his paper, and I am afraid that the variety has entirely disappeared; for a short time after these sheep had become prevalent in that district the Merino sheep were introduced; and as their wool was much more valuable, and as they were a quiet race of sheep, and showed no tendency to trespass or jump over fences, the Otter breed of sheep, the wool of which was inferior to that of the Merino, was gradually allowed to die out.

You see that these facts illustrate perfectly well what may be done if you take care to breed from stocks that are similar to each other. After having got a variation, if, by crossing a variation with the original stock, you multiply that variation, and then take care to keep that variation distinct from the original stock, and make them breed together—then you may almost certainly produce a race whose tendency to continue the variation is exceedingly strong.

This is what is called "selection;" and it is by exactly the same process as that by which Seth Wright bred his Ancon sheep that our breeds of cattle, dogs, and fowls are obtained. There are some possibilities of exception, but still, speaking broadly, I may say that this is the way in which all our varied races of domestic animals have arisen; and you must understand that it is not one peculiarity or one characteristic alone in which animals may vary. There is not a single peculiarity or characteristic of any kind, bodily or mental, in which offspring may not vary to a certain extent from the parent and other animals.

Among ourselves this is well known. The simplest physical peculiarity is mostly reproduced. I know a case of a man whose wife has the lobe of one of her ears a little flattened. An ordinary observer might scarcely notice it, and yet every one of her children has an approximation to the same peculiarity to some extent.

If you look at the other extreme, too, the gravest diseases, such as gout, scrofula, and consumption, may be handed down with just the same certainty and persistence as we noticed in the perpetuation of the bandy legs of the Ancon sheep.

However, these facts are best illustrated in animals, and the extent of the variation, as is well known, is very remarkable in dogs. For example, there are some dogs very much smaller than others; indeed, the variation is so enormous that probably the smallest dog would be about the size of the head of the largest; there are very great variations in the structural forms not only of the skeleton but also in the shape of the skull, and in the proportions of the face and the disposition of the teeth.

The pointer, the retriever, bulldog, and the terrier, differ very greatly, and yet there is every reason to believe that every one of these races has arisen from the same source—that all the most important races have arisen by this selective breeding from accidental variation.

A still more striking case of what may be done by selective breeding, and it is a better case, because there is no chance of that partial infusion of error to which I allude, has been studied very carefully by Mr. Darwin—the case of the domestic pigeons. I dare say there may be some among you who may be pigeon *fanciers*, and I wish you to understand that in approaching the subject I would speak with all humility and hesitation, as I regret to say that I am not a pigeon fancier. I know it is a great art and mystery, and a thing upon which a man must not speak lightly; but I shall endeavor, as far as my understanding goes, to give you a summary of the published and unpublished information which I have gained from Mr. Darwin.

Among the enormous variety—I believe there are somewhere about a hundred and fifty kinds of pigeons—there are four kinds which may be selected as representing the extremest divergences of one kind from another. Their names are the carrier, the pouter, the fantail, and the tumbler. In these large diagrams that I have here they are each represented in their relative sizes to each other. This first one is the carrier; you will notice this large excrescence on its beak; it has a comparatively small head; there is a bare space round the eyes; it has a long neck, a very long beak, very strong legs, large feet, long wings, and so on. The second one is the pouter, a very large bird, with very long legs and beak. It is called the pouter because it is in the habit of causing its gullet to swell up by inflating it with air. I should tell you that all pigeons have a tendency to do this at times, but in the pouter it is carried to an enormous extent. The birds appear to be quite proud of their power of swelling and puffing themselves out in this way; and I think it is about as droll a sight as you can well see to look at a cage full of these pigeons puffing and blowing themselves out in this ridiculous manner.

This diagram is a representation of the third kind I mentioned—the fantail. It is, you see, a small bird, with exceedingly small legs and a very small beak. It is most curi-

ously distinguished by the size and extent of its tail, which, instead of containing fourteen feathers, may have many more—say thirty, or even more—I believe there are some with as many as forty-two. This bird has a curious habit of spreading out the feathers of its tail in such a way that they reach forward and touch its head; and if this can be accomplished I believe it is looked upon as a point of great beauty.

But here is the last great variety—the tumbler; and of that great variety, one of the principal kinds, and one most prized, is the specimen represented here—the short-faced tumbler. Its beak, you see, is reduced to a mere nothing. Just compare the beak of this one and that of the first one, the carrier—I believe the orthodox comparison of the head and beak of a thoroughly well-bred tumbler is to stick an oat into a cherry, and that will give you the proper relative proportions of the head and beak. The feet and legs are exceedingly small, and the bird appears to be quite a dwarf when placed side by side with this great carrier.

These are differences enough in regard to their external apperance; but these differences are by no means the whole or even the most important of the differences which obtain between these birds. There is hardly a single point of their structure which has not become more or less altered; and to give you an idea of how extensive these alterations are, I have here some very good skeletons, for which I am indebted to my friend Mr. Tegetmeier, a great authority in these matters, by means of which, if you examine them by and by, you will be able to see the enormous difference in their bony structures.

I had the privilege, some time ago, of access to some important MSS. of Mr. Darwin, who, I may tell you, has taken very great pains and spent much valuable time and attention on the investigation of these variations, and getting together all the facts that bear upon them. I obtained from these MSS. the following summary of the differences between the domestic breeds of pigeons—that is to say, a notification of the various points in which their organization differs. In the first place, the back of the skull may differ a good deal, and the development of the bones of the face may vary a great deal; the back varies a good deal; the shape of the lower jaw varies; the tongue varies very greatly not only in correlation to the length and size of the beak, but it seems also to have a kind of independent variation of its own. Then the amount of naked skin round the eyes, and at the base of the beak, may vary enormously; so may the length of the eyelids, the shape of the nostrils, and the length of the neck. I have already noticed the habit of blowing out the gullet, so remarkable in the pouter, and comparatively so in the others. There are great differences, too, in the size of the female and the male, the shape of the body, the number and width of the processes of the ribs, the development of the ribs, and the size, shape, and development of the breast-bone. We may notice, too—and I mention the fact because it has been disputed by what is assumed to be high authority—the variation in number of the sacral vertebræ. The number of these varies form eleven to fourteen, and that without any diminution in the number of the vertebræ of the back or of the tail. Then the number and position of the tail-feathers may vary enormously, and so may the number of the primary and secondary feathers of the wings. Again, the length of the feet and of the beak—although they have no relation to each other, yet appear to go together—that is, you have a long beak wherever you have long feet. There are differences also in the periods of the acquirement of the perfect plumage—the size and shape of the eggs—the nature of flight, and the powers of flight—so-called "*homing*" birds having enormous flying powers;* while, on the other hand, the little tumbler is so called because of its extraordinary faculty of turning head over heels in the air, instead of pursuing a distinct course. And, lastly, the dispositions and voices of the birds may vary. Thus the case of the pigeons shows you that there is hardly a single particular—whether of instinct, or habit, or bony structure, or of plumage—of either the internal economy or the external shape, in which some variation or change may not take place which by selective breeding may become perpetuated, and form the foundation of, and give rise to, a new race.

If you carry in your mind's eye these four varieties of pigeons, you will bear with you as good a notion a you can have, perhaps, of the enormous extent to which a deviation from a primitive type may be carried by means of this process of selective breeding.

LECTURE V.

THE CONDITIONS OF EXISTENCE AS AFFECTING THE PERPETUATION OF LIVING BEINGS.

In the last lecture I endeavored to prove to you that while, as a general rule, organic beings tend to reproduce their kind, there is in them, also, a constantly recurring tendency to vary—to vary to a greater or to a less extent. Such a variety, I pointed out to you, might arise from causes which we do not understand; we therefore called it spontaneous; and it might come into existence as a definite and marked thing, without any gradations between itself and the form which preceded it. I further pointed out that such a variety having once arisen, might be perpetuated to some extent, and indeed to a very marked extent, without any direct interference, or without any exercise of that process which we called selection. And then I stated further that by such selection, when exercised artificially—if you took care to breed only from those forms which presented the

* The "*Carrier*," I learn from Mr. Tegetmeier, does not *carry*, a high-bred bird of this breed being but a poor flyer. The birds which fly long distances, and come home—"homing" birds—and are consequently used as carriers, are not "carriers" in the fancy sense.

same peculiarities of any variety which had arisen in this manner—the variation might be perpetuated, as far as we can see, indefinitely.

The next question, and it is an important one for us, is this: Is there any limit to the amount of variation from the primitive stock which can be produced by this process of selective breeding? In considering this question, it will be useful to class the characteristics, in respect of which organic beings vary, under two heads: we may consider structural characteristics, and we may consider physiological characteristics.

In the first place, as regards structural characteristics, I endeavored to show you, by the skeletons which I had upon the table, and by reference to a great many well-ascertained facts, that the different breeds of pigeons, the carriers, pouters, and tumblers, might vary in any of their internal and important structural characters to a very great degree; not only might there be changes in the proportions of the skull, and the characters of the feet and beaks, and so on, but that there might be an absolute difference in the number of the vertebræ of the back, as in the sacral vertebræ of the pouter; and so great is the extent of the variation in these and similar characters that I pointed out to you, by reference to the skeletons and the diagrams, that these extreme varieties may absolutely differ more from one another in their structural characters than do what naturalists call distinct SPECIES of pigeons; that is to say, that they differ so much in structure that there is a greater difference between the pouter and the tumbler than there is between such wild and distinct forms as the rock pigeon or the ring pigeon, or the ring pigeon and the stock dove; and, indeed, the differences are of greater value than this, for the structural differences between these domesticated pigeons are such as would be admitted by a naturalist, supposing he knew nothing at all about their origin, to entitle them to constitute even distinct genera.

As I have used this term SPECIES, and shall probably use it a good deal, I had better, perhaps, devote a word or two to explaining what I mean by it.

Animals and plants are divided into groups, which become gradually smaller, beginning with a kingdom, which is divided into sub-kingdoms; then come the smaller divisions called provinces; and so on from a province to a class, from a class to an order, from orders to families, and from these to genera, until we come at length to the smallest groups of animals which can be defined one from the other by constant characters, which are not sexual; and these are what naturalists call species in practice, whatever they may do in theory.

If in a state of nature you find any two groups of living beings which are separated one from the other by some constantly-recurring characteristic, I don't care how slight and trivial, so long as it is defined and constant, and does not depend on sexual peculiarities, then all naturalists agree in calling them two species; that is what is meant by the use of the word species—that is to say, it is for the practical naturalist a mere question of structural differences.*

We have seen now—to repeat this point once more, and it is very essential that we should rightly understand it—we have seen that breeds, known to have been derived from a common stock by selection, may be as different in their structure from the original stock as species may be distinct from each other.

But is the like true of the physiological characteristics of animals. Do the physiological differences of varieties amount in degree to those observed between forms which naturalists call distinct species? This is a most important point for us to consider.

As regards the great majority of physiological characteristics, there is no doubt that they are capable of being developed, increased, and modified by selection.

There is no doubt that breeds may be made as different as species in many physiological characters. I have already pointed out to you very briefly the different habits of the breeds of pigeons, all of which depend upon their physiological peculiarities—as the peculiar habit of tumbling, in the tumbler—the peculiarities of flight, in the "homing" birds—the strange habit of spreading out the tail, and walking in a peculiar fashion, in the fantail—and, lastly, the habit of blowing out the gullet, so characteristic of the pouter. These are all due to physiological modification, and in all these respects these birds differ as much from each other as any two ordinary species do.

So with dogs in their habits and instincts. It is a physiological peculiarity which leads the greyhound to chase its prey by sight—that enables the beagle to track it by the scent—that impels the terrier to its rat-hunting propensity—and that leads the retriever to its habit of retrieving. These habits and instincts are all the results of physiological differences and peculiarities, which have been developed from a common stock, at least there is every reason to believe so. But it is a most singular circumstance that while you may run through almost the whole series of physiological processes, without finding a check to your argument, you come at last to a point where you find a check, and that is in the reproductive processes. For there is a most singular circumstance in respect to natural species—at least about some of them—and it would be sufficient for the purposes of this argument if it were true of only one of them, but there is, in fact, a great number of such cases—and that is that, similar as they may appear to be to mere races or breeds, they present a marked peculiarity in the reproductive process. If you breed from the male and female of the same race, you of

* I lay stress here on the *practical* signification of "Species." Whether a physiological test between species exists or not, it is hardly ever applicable by the practical naturalist.

course have offspring of the like kind, and if you make the offspring breed together, you obtain the same result, and if you breed from these again, you will still have the same kind of offspring; there is no check. But if you take members of two distinct species, however similar they may be to each other, and make them breed together, you will find a check, with some modifications and exceptions, however, which I shall speak of presently. If you cross two such species with each other, then—although you may get offspring in the case of the first cross, yet, if you attempt to breed from the products of that crossing, which are what are called hybrids—that is, if you couple a male and a female hybrid—then the result is that in ninety-nine cases out of a hundred you will get no offspring at all; there will be no result whatsoever.

The reason of this is quite obvious in some cases; the male hybrids, although possessing all the eternal appearances and characteristics of perfect animals, are physiologically imperfect and deficient in the structural parts of the reproductive elements necessary to generation. It is said to be invariably the case with the male mule, the cross between the ass and the mare; and hence it is that, although crossing the horse with the ass is easy enough, and is constantly done as far as I am aware, if you take two mules, a male and a female, and endeavor to breed from them, you get no offspring whatever; no generation will take place. This is what is called the sterility of the hybrids between two distinct species.

You see that this is a very extraordinary circumstance; one does not see why it should be. The common teleological explanation is, that it is to prevent the impurity of the blood resulting from the crossing of one species with another, but you see it does not in reality do anything of the kind. There is nothing in this fact that hybrids cannot breed with each other to establish such a theory; there is nothing to prevent the horse breeding with the ass, or the ass with the horse. So that this explanation breaks down, as a great many explanations of this kind do, that are only founded on mere assumptions.

Thus you see that there is a great difference between "mongrels," which are crosses between distinct races, and "hybrids," which are crosses between distinct species. The mongrels are, so far as we know, fertile with one another. But between species, in many cases, you cannot succeed in obtaining even the first cross; at any rate it is quite certain that the hybrids are often absolutely infertile one with another.

Here is a feature, then, great or small as it may be, which distinguishes natural species of animals. Can we find any approximation to this in the different races known to be produced by selective breeding from a common stock? Up to the present time the answer to that question is absolutely a negative one. As far as we know at present, *there is nothing approximating* to this check. In crossing the breeds between the fantail and the pouter, the carrier and the tumbler, or any other variety or race you may name—so far as we know at present—there is no difficulty in breeding together the mongrels. Take the carrier and the fantail, for instance, and let them represent the horse and the ass in the case of distinct species; then you have, as the result of their breeding, the carrier-fantail mongrel—we will say the male and female mongrel—and, as far as we know, these two when crossed would not be less fertile than the original cross, or than carrier with carrier. Here, you see, is a physiological contrast between the races produced by selective modification and natural species. I shall inquire into the value of this fact, and of some modifying circumstances, by and by; for the present I merely put it broadly before you.

But while considering this question of the limitations of species, a word must be said about what is called Recurrence—the tendency of races which have been developed by selective breeding from varieties to return to their primitive type. This is supposed by many to put an absolute limit to the extent of selective and all other variations. People say, "It is all very well to talk about producing these different races, but you know very well that if you turned all these birds wild, these pouters and carriers, and so on, they would all return to their primitive stock." This is very commonly assumed to be a fact, and it is an argument that is commonly brought forward as conclusive; but if you will take the trouble to inquire into it rather closely, I think you will find that it is not worth very much. The first question of course is, Do they thus return to the primitive stock? And commonly as the thing is assumed and accepted, it is extremely difficult to get anything like good evidence of it. It is constantly said, for example, that if domesticated horses are turned wild, as they have been in some parts of Asia Minor and South America, that they return at once to the primitive stock from which they were bred. But the first answer that you make to this assumption is, to ask who knows what the primitive stock was; and the second answer is, that in that case the wild horses of Asia Minor ought to be exactly like the wild horses of South America. If they are both like the same thing, they ought manifestly to be like each other! The best authorities, however, tell you that it is quite different. The wild horse of Asia is said to be of a dun color, with a largish head, and a great many other peculiarities, while the best authorities on the wild horses of South America tell you that there is nothing of this sort in the wild horses there; the cut of their heads is very different, and they are commonly chestnut or bay colored. It is quite clear, therefore, that as by these facts there ought to have been two primitive stocks, they go for nothing in support of the assumption that races recur to one primitive stock, and so far as this evidence is con-

cerned it falls to the ground.

Suppose for a moment that it were so, and that domesticated races, when turned wild, did return to some common condition, I cannot see that this would prove much more than that similar conditions are likely to produce similar results; and that when you take back domesticated animals into what we call natural conditions, you do exactly the same thing as if you carefully undid all the work you had gone through, for the purpose of bringing the animal from its wild to its domesticated state. I do not see anything very wonderful in the fact, if it took all that trouble to get it from a wild state, that it should go back into its original state as soon as you remove the conditions which produced the variation to the domesticated form. There is an important fact, however, forcibly brought forward by Mr. Darwin, which has been noticed in connection with the breeding of domesticated pigeons; and it is that, however different these breeds of pigeons may be from each other, and we have already noticed the great differences in these breeds, that if, among any of those variations, you chance to have a blue pigeon turn up, it will be sure to have the black bars across the wings, which are characteristic of the original wild stock, the rock pigeon.

Now this is certainly a very remarkable circumstance: but I do not see myself how it tells very strongly either one way or the other. I think, in fact, that this argument in favor of recurrence to the primitive type might prove a great deal too much for those who so constantly bring it forward. For example, Mr. Darwin has very forcibly urged that nothing is commoner than if you examine a dun horse—and I had an opportunity of verifying this illustration lately while in the islands of the West Highlands, where there are a great many dun horses—to find that horse exhibit a long black stripe down his back, very often stripes on his shoulder, and very often stripes on his legs. I myself saw a pony of this description a short time ago, in a baker's cart, near Rothesay, in Bute; it had the long stripe down the back, and stripes on the shoulders and legs, just like those of the ass, the quagga, and the zebra. Now if we interpret the theory of recurrence as applied to this case, might it not be said that here was a case of a variation exhibiting the characters and conditions of an animal occupying something like an intermediate position between the horse, the ass, the quagga, and the zebra, and from which these had been developed? In the same way with regard even to man. Every anatomist will tell you that there is nothing commoner, in dissecting the human body, than to meet with what are called muscular variations—that is, if you dissect two bodies very carefully you will probably find that the modes of attachment and insertion of the muscles are not exactly the same in both, there being great peculiarities in the mode in which the muscles are arranged; and it is very singular that in some dissections of the human body you will come upon arrangements of the muscles very similar indeed to the same parts in the apes. Is the conclusion in that case to be that this is like the black bars in the case of the pigeon, and that it indicates a recurrence to the primitive type from which the animals have been probably developed? Truly, I think that the opponents of modification and variation had better leave the argument of recurrence alone, or it may prove altogether too strong for them.

To sum up—the evidence as far as we have gone is against the argument as to any limit to divergences, so far as structure is concerned; and in favor of a physiological limitation. By selective breeding we can produce structural divergences as great as those of species, but we cannot produce equal physiological divergences. For the present I leave the question there.

Now the next problem that lies before us—and it is an extremely important one—is this: Does this selective breeding occur in nature? Because, if there is no proof of it, all that I have been telling you goes for nothing in accounting for the origin of species. Are natural causes competent to play the part of selection in perpetuating varieties? Here we labor under very great difficulties. In the last lecture I had occasion to point out to you the extreme difficulty of obtaining evidence even of the first origin of those varieties which we know to have occurred in domesticated animals. I told you that almost always the origin of these varieties is overlooked, so that I could only produce two or three cases, as that of Gratio Kelleia and of the Ancon sheep. People forget, or do not take notice of them until they come to have a prominence; and if that is true of artificial cases, under our own eyes, and in animals in our own care, how much more difficult it must be to have at first hand good evidence of the origin of varieties in nature! Indeed, I do not know that it is possible by direct evidence to prove the origin of a variety in nature, or to prove selective breeding; but I will tell you what we can prove—and this comes to the same thing—that varieties exist in nature within the limits of species, and, what is more, that when a variety has come into existence in nature there are natural causes and conditions which are amply competent to play the part of a selective breeder; and although that is not quite the evidence that one would like to have—though it is not direct testimony—yet it is exceeding good and exceedingly powerful evidence in its way.

As to the first point, of varieties existing among natural species, I might appeal to the universal experience of every naturalist, and of any person who has ever turned any attention at all to the characteristics of plants and animals in a state of nature; but I may as well take a few definite cases, and I will begin with man himself.

I am one of those who believe that, at present, there is no evidence whatever for saying that mankind sprang originally from

any more than a single pair; I must say that I cannot see any good ground whatever, or even any tenable sort of evidence, for believing that there is more than one species of man. Nevertheless, as you know, just as there are numbers of varieties in animals, so there are remarkable varieties of men. I speak not merely of those broad and distinct variations which you see at a glance. Everybody, of course, knows the difference between a negro and a white man, and can tell a Chinaman from an Englishman. They each have peculiar characteristics of color and physiognomy; but you must recollect that the characters of these races go very far deeper—they extend to the bony structure, and to the characters of that most important of all organs to us—the brain; so that, among men belonging to different races, or even within the same race, one man shall have a brain a third, or half, or even seventy per cent bigger than another; and if you take the whole range of human brains, you will find a variation in some cases of a hundred per cent. Apart from these variations in the size of the brain the characters of the skull vary. Thus if I draw the figures of a Mongul and a negro head on the blackboard, in the case of the last the breadth would be about seven tenths, and in the other it would be nine tenths of the total length. So that you see there is abundant evidence of variation among men in their natural condition. And if you turn to other animals there is just the same thing. The fox, for example, which has a very large geographical distribution all over Europe, and parts of Asia, and on the American Continent, varies greatly. There are mostly large foxes in the North, and smaller ones in the South. In Germany alone the foresters reckon some eight different sorts.

Of the tiger, no one supposes that there is more than one species; they extend from the hottest parts of Bengal into the dry, cold, bitter steppes of Siberia, into a latitude of 50°—so that they may even prey upon the reindeer. These tigers have exceedingly different characteristics, but still they all keep their general features, so that there is no doubt as to their being tigers. The Siberian tiger has a thick fur, a small mane, and a longitudinal stripe down the back, while the tigers of Java and Sumatra differ in many important respects from the tigers of Northern Asia. So lions vary; so birds vary; and so, if you go farther back and lower down in creation, you find fishes vary. In different streams, in the same country even, you will find the trout to be quite different to each other and easily recognizable by those who fish in the particular streams. There is the same differences in leeches; leech collectors can easily point out to you the differences and the peculiarities which you yourself would probably pass by; so with fresh-water mussels; so, in fact, with every animal you can mention.

In plants there is the same kind of variation. Take such a case even as the common bramble. The botanists are all at war about it, some of them wanting to make out that there are many species of it, and others maintaining that they are but many varieties of one species; and they cannot settle to this day which is a species and which is a variety!

So that there can be no doubt whatsoever that any plant and any animal may vary in nature: that varieties may arise in the way I have described—as spontaneous varieties—and that those varieties may be perpetuated in the same way that I have shown you spontaneous varieties are perpetuated; I say, therefore, that there can be no doubt as to the origin and perpetuation of varieties in nature.

But the question now is, Does selection take place in nature? is there anything like the operation of man in exercising selective breeding taking place in nature? You will observe that, at present, I say nothing about species; I wish to confine myself to the consideration of the production of those natural races which everybody admits to exist. The question is, whether in nature there are causes competent to produce races, just in the same way as man is able to produce, by selection, such races of animals as we have already noticed.

When a variety has arisen, the Conditions of Existence are such as to exercise an influence which is exactly comparable to that of artificial selection. By Conditions of Existence I mean two things—there are conditions which are furnished by the physical, the inorganic world, and there are conditions of existence which are furnished by the organic world. There is, in the first place, Climate; under that head I include only temperature and the varied amount of moisture of particular places. In the next place there is what is technically called Station, which means—given the climate, the particular kind of place in which an animal or a plant lives or grows; for example, the station of a fish is in the water, of a fresh water fish in fresh water; the station of a marine fish is in the sea, and a marine animal may have a station higher or deeper. So again with land animals; the differences in their stations are those of different soils and neighborhoods, some being best adapted to a calcareous, and others to an arenaceous oil. The third condition of existence is Food, by which I mean food in the broadest sense, the supply of the materials necessary to the existence of an organic being; in the case of a plant the inorganic matters, such as carbonic acid, water, ammonia, and the earthly salts or salines; in the case of the animal the inorganic and organic matters, which we have seen they require; then these are all, at least the two first, what we may call the inorganic or physical conditions of existence. Food takes a mid-place, and then come the organic conditions, by which I mean the conditions which depend upon the state of the rest of the organic creation, upon the number and kind of living beings, with which an animal is surrounded. You may class these under two heads: there are organic beings, which

operate as *opponents*, and there are organic beings, which operate as *helpers* to any given organic creature. The opponents may be of two kinds: there are the *indirect opponents*, which are what we may call *rivals*; and there are the *direct opponents*, those which strive to destroy the creature; and these we call *enemies*. By rivals I mean, of course, in the case of plants, those which require for their support the same kind of soil and station, and, among animals, those which require the same kind of station, or food, or climate; those are the indirect opponents; the direct opponents are, of course, those which prey upon an animal or vegetable. The *helpers* may also be regarded as direct and indirect: in the case of a carnivorous animal, for example, a particular herbaceous plant may in multiplying be an indirect helper by enabling the herbivora on which the carnivore preys to get more food, and thus to nourish the carnivore more abundantly; the direct helper may be best illustrated by reference to some parasitic creature, such as the tape-worm. The tape-worm exists in the human intestines, so that the fewer there are of men the fewer there will be of tape-worms, other things being alike. It is a humiliating reflection, perhaps, that we may be classed as direct helpers to the tape-worm, but the fact is so; we can all see that if there were no men there would be no tape-worms.

It is extremely difficult to estimate, in a proper way, the importance and the working of the Conditions of Existence. I do not think there were any of us who had the remotest notion of properly estimating them until the publication of Mr. Darwin's work, which has placed them before us with remarkable clearness; and I must endeavor, as far as I can in my own fashion, to give you some notion of how they work. We shall find it easiest to take a simple case, and one as free as possible from every kind of complication.

I will suppose, therefore, that all the habitable part of this globe—the dry land, amounting to about 51,000,000 square miles —I will suppose that the whole of that dry land has the same climate, and that it is composed of the same kind of rock or soil, so that there will be the same station everywhere; we thus get rid of the peculiar influence of different climates and stations. I will then imagine that there shall be but one organic being in the world, and that shall be a plant. In this we start fair. Its food is to be carbonic acid, water, and ammonia, and the saline matters in the soil, which are, by the supposition, everywhere alike. We take one single plant, with no opponents, no helpers, and no rivals; it is to be a "fair field and no favor." Now I will ask you to imagine further that it shall be a plant which shall produce every year fifty seeds, which is a very moderate number for a plant to produce; and that, by the action of the winds and currents, these seeds shall be equally and gradually distributed over the whole surface of the land. I want you now to trace out what will occur, and you will observe that I am not talking fallaciously any more than a mathematician does when he expounds his problem. If you show that the conditions of your problem are such as may actually occur in nature, and do not transgress any of the known laws of nature in working out your proposition, then you are as safe in the conclusion you arrive at as is the mathematician in arriving at the solution of his problem. In science, the only way of getting rid of the complications with which a subject of this kind is environed is to work in this deductive method. What will be the result then? I will suppose that every plant requires one square foot of ground to live upon; and the result will be that, in the course of nine years, the plant will have occupied every single available spot in the whole globe! I have chalked upon the blackboard the figures by which I arrive at the result:

Plants.					Plants.
1 × 50	in	1st	year	=	50
50 × 50	"	2d	"	=	2,500
2,500 × 50	"	3d	"	=	125,000
125,000 × 50	"	4th	"	=	6,250,000
6,250,000 × 50	"	5th	"	=	312,500,000
312,500,000 × 50	"	6th	"	=	15,625,000,000
15,625,000,000 × 50	"	7th	"	=	781,250,000,000
781,250,000,000 × 50	"	8th	"	=	39,062,500,000,000
39,062,500,000,000 × 50	"	9th	"	=	1,953,125,000,000,000
51,000,000 sq. miles—the dry surface of the earth × 27,878,400—the number of sq. ft. in 1 sq. mile.				= sq. ft.	1,421,798,400,000,000
					being 531,326,600,000,000

square feet less than would be required at the end of the ninth year.

You will see from this that, at the end of the first year, the single plant will have produced fifty more of its kind; by the end of the second year these will have increased to 2500; and so on, in succeeding years, you get beyond even trillions; and I am not at all sure that I could tell you what the proper arithmetical denomination of the total number really is; but, at any rate, you will understand the meaning of all those noughts. Then you see that, at the bottom, I have taken the 51,000,000 of square miles, constituting the surface of the dry land; and as the number of square feet are placed under and abstracted from the number of seeds that would be produced in the ninth year, you can see at once that there would be an immense number more of plants than there would be square feet of ground for their accommodation. This is certainly quite enough to prove my point; that between the eight and ninth year after being planted the single plant

would have stocked the whole available surface of the earth.

This is a thing which is hardly conceivable—it seems hardly imaginable—yet it is so. It is indeed simply the law of Malthus exemplified. Mr. Malthus was a clergyman, who worked out this subject most minutely and truthfully some years ago; he showed quite clearly—and although he was much abused for his conclusions at the time, they have never yet been disproved, and never will be—he showed that in consequence of the increase in the number of organic beings in a geometrical ratio while the means of existence cannot be made to increase in the same ratio, that there must come a time when the number of organic beings will be in excess of the power of production of nutriment, and that thus some check must arise to the further increase of those organic beings. At the end of the ninth year we have seen that each plant would not be able to get its full square foot of ground, and at the end of another year it would have to share that space with fifty others the produce of the seeds which it would give off.

What, then, takes place? Every plant grows up, flourishes, occupies its square foot of ground, and gives off its fifty seeds; but notice this, that out of this number only one can come to anything; there is thus, as it were, forty-nine chances to one against its growing up; it depends upon the most fortuitous circumstances whether any one of these fifty seeds shall grow up and flourish, or whether it shall die and perish. This is what Mr. Darwin has drawn attention to, and called the "Struggle for Existence;" and I have taken this simple case of a plant because some people imagine that the phrase seems to imply a sort of fight.

I have taken this plant and shown you that this is the result of the ratio of the increase, the necessary result of the arrival of a time coming for every species when exactly as many members must be destroyed as are born; that is the inevitable ultimate result of the rate of production. Now, what is the result of all this? I have said that there are forty-nine struggling against every one; and it amounts to this, that the smallest possible start given to any one seed may give it an advantage which will enable it to get ahead of all the others; anything that will enable any one of these seeds to germinate six hours before any of the others will, other things being alike, enable it to choke them out altogether. I have shown you that there is no particular in which plants will not vary from each other; it is quite possible that one of our imaginary plants may vary in such a character as the thickness of the integument of its seeds. It might happen that one of the plants might produce seeds having a thinner integument, and that would enable the seed of that plant to germinate a little quicker than those of any of the others, and those seeds would most inevitably extinguish the forty-nine times as many that were struggling with them.

I have put it in this way, but you see the practical result of the process is the same as if some person had nurtured the one and destroyed the other seeds. It does not matter how the variation is produced, so long as it is once allowed to occur. The variation in the plant once fairly started tends to become hereditary and reproduce itself; the seeds would spread themselves in the same way and take part in the struggle with the forty-nine hundred, or forty-nine thousand, with which they might be exposed. Thus, by degrees, this variety, with some slight organic change or modification, must spread itself over the whole surface of the habitable globe, and extirpate or replace the other kinds. That is what is meant by Natural Selection; that is the kind of argument by which it is perfectly demonstrable that the conditions of existence may play exactly the same part for natural varieties as man does for domesticated varieties. No one doubts at all that particular circumstances may be more favorable for one plant and less so for another, and the moment you admit that you admit the selective power of nature. Now, although I have been putting a hypothetical case, you must not suppose that I have been reasoning hypothetically. There are plenty of direct experiments which bear out what we may call the theory of natural selection; there is extremely good authority for the statement that if you take the seed of mixed varieties of wheat and sow it, collecting the seed next year and sowing it again, at length you will find that out of all your varieties only two or three have lived, or perhaps even only one. There were one or two varieties which were best fitted to get on, and they have killed out the other kinds in just the same way and with just the same certainty as if you had taken the trouble to remove them. As I have already said, the operation of nature is exactly the same as the artificial operation of man.

But if this be true of that simple case, which I put before you, where there is nothing but the rivalry of one member of a species with others, what must be the operation of selective conditions, when you recollect as a matter of fact that for every species of animal or plant there are fifty or a hundred species which might all, more or less, be comprehended in the same climate, food, and station—that every plant has multitudinous animals which prey upon it, and which are its direct opponents; and that these have other animals preying upon them—that every plant has its indirect helpers in the birds that scatter abroad its seed, and the animals that manure it with their dung; I say, when these things are considered, it seems impossible that any variation which may arise in a species in nature should not tend in some way or other either to be a little better or worse than the previous stock; if it is a little better it will have an advantage over and tend to extirpate the latter in this crush and struggle; and if it is a little worse it will itself be extirpated.

I know nothing that more appropriately expresses this than the phrase, "the struggle for existence;" because it brings before your minds, in a vivid sort of way, some of the simplest possible circumstances connected with it. When a struggle is intense there must be some who are sure to be trodden down, crushed, and overpowered by others; and there will be some who just manage to get through only by the help of the slightest accident. I recollect reading an account of the famous retreat of the French troops, under Napoleon, from Moscow. Worn out, tired, and dejected, they at length came to a great river over which there was but one bridge for the passage of the vast army. Disorganized and demoralized as it was, the struggle must certainly have been a terrible one—every one heeding only himself, and crushing through and treading down his fellows. The writer of the narrative, who was himself one of those who were fortunate enough to succeed in getting over, and not among the thousands who were left behind or forced into the river, ascribed his escape to the fact that he saw striding onward through the mass a great strong fellow—one of the French cuirassiers who had on a large blue cloak—and he had enough presence of mind to catch and retain a hold of this strong man's cloak. He says, "I caught hold of his cloak, and although he swore at me and cut at and struck me by turns, and at last, when he found he could not shake me off, fell to entreating me to leave go or I should prevent him from escaping, besides not assisting myself, I still kept tight hold of him, and would not quit my grasp until he had at last dragged me through." Here you see was a case of selective saving—if we may so term it—depending for its success on the strength of the cloth of the cuirassier's cloak. It is the same in nature; every species has its Beresina; it has to fight its way through and struggle with other species; and when well-nigh overpowered, it may be that the smallest chance, something in its color, perhaps—the minutest circumstance—will turn the scale one way or the other.

Suppose that by a variation of the black race it had produced the white man at any time—you know that the negroes are said to believe this to have been the case, and to imagine that Cain was the first white man, and that we are his descendants—suppose that this had ever happened, and that the first residence of this human being was on the west coast of Africa. There is no great structural difference between the white man and the negro, and yet there is something so singularly different in the constitution of the two, that the malarias of that country, which do not hurt the black at all, cut off and destroy the white, thus you see there would have been a selective operation performed. If the white man had risen in that way he would have been selected out and removed by means of the malaria. Now there really is a very curious case of selection of this sort among pigs, and it is a case of selection of color too. In the woods of Florida there are a great many pigs, and it is a very curious thing that they are all black, every one of them. Professor Wyman was there some years ago, and on noticing no pigs but these black ones, he asked some of the people how it was that they had no white pigs, and the reply was that in the woods of Florida there was a root which they called the paint root, and that if the white pigs were to eat any of it, it had the effect of making their hoofs crack, and they died; but if the black pigs eat any of it, it did not hurt them at all. Here was a very simple case of natural selection. A skilful breeder could not more carefully develop the black breed of pigs, and weed out all the white pigs, than the paint root does.

To show you how remarkably indirect may be such natural selective agencies as I have referred to, I will conclude by noticing a case mentioned by Mr. Darwin, and which is certainly one of the most curious of its kind. It is that of the humble bee. It has been noticed that there are a great many more humble bees in the neighborhood of towns than out in the open country, and the explanation of the matter is this: the humble bees build nests in which they store their honey and deposit the larvæ and eggs. The field mice are amazingly fond of the honey and larvæ; therefore, wherever there are plenty of field mice, as in the country, the humble bees are kept down; but in the neighborhood of towns, the number of cats which prowl about the fields eat up the field mice, and of course the more mice they eat up the less there are to prey upon the larvæ of the bees—the cats are therefore the *indirect helpers* of the bees.* Coming back a step farther, we may say that the old maids are also indirect friends of the humble bees, and indirect enemies of the field mice, as they keep the cats which eat up the latter. This is an illustration somewhat beneath the dignity of the subject, perhaps, but it occurs to me in passing, and with it I will conclude this lecture.

LECTURE VI.

A CRITICAL EXAMINATION OF THE POSITION OF MR. DARWIN'S WORK, "ON THE ORIGIN OF SPECIES," IN RELATION TO THE COMPLETE THEORY OF THE CAUSES OF THE PHENOMENA OF ORGANIC NATURE.

In the preceding five lectures I have endeavored to give you an account of those facts, and of those reasonings from facts, which form the data upon which all theories regarding the causes of the phenomena of organic nature must be based. And, although I have had frequent occasion to quote Mr. Darwin—as all persons hereafter,

* The humble bees, on the other hand, are direct helpers of some plants, such as the heartsease and red clover, which are fertilized by the visits of the bees; and they are indirect helpers of the numerous insects which are more or less completely supported by the heartsease and red clover.

in speaking upon these subjects, will have occasion to quote his famous book on the "Origin of Species"—you must yet remember that, wherever I have quoted him, it has not been upon theoretical points, or for statements in any way connected with his particular speculations, but on matters of fact, brought forward by himself, or collected by himself, and which appear incidentally in his book. If a man *will* make a book, professing to discuss a single question, an encyclopædia, I cannot help it.

Now, having had an opportunity of considering in this sort of way the different statements bearing upon all theories whatsoever, I have to-night to lay before you, as fairly as I can, what is Mr. Darwin's view of the matter and what position his theories hold, when judged by the principles which I have previously laid down as deciding our judgments upon all theories and hypotheses.

I have already stated to you that the inquiry respecting the causes of the phenomena of organic nature resolves itself into two problems, the first being the question of the origination of living or organic beings, and the second being the totally distinct problem of the modification and perpetuation of organic beings when they have already come into existence. The first question Mr. Darwin does not touch; he does not deal with it at all; but he says—given the origin of organic matter—supposing its creation to have already taken place, my object is to show in consequence of what laws and what demonstrable properties of organic matter, and of its environments, such states of organic nature as those with which we are acquainted must have come about. This, you will observe, is a perfectly legitimate proposition; every person has a right to define the limits of the inquiry which he sets before himself; and yet it is a most singular thing that in all the multifarious and not unfrequently ignorant attacks which have been made upon the "Origin of Species," there is nothing which has been more speciously criticised than this particular limitation. If people have nothing else to urge against the book, they say, "Well, after all, you see Mr. Darwin's explanation of the 'Origin of Species' is not good for much, because, in the long run, he admits that he does not know how organic matter began to exist. But if you admit any special creation for the first particle of organic matter, you may just as well admit it for all the rest; five hundred or five thousand distinct creations are just as intelligible, and just as little difficult to understand, as one." The answer to these cavils is twofold. In the first place, all human inquiry must stop somewhere; all our knowledge and all our investigation cannot take us beyond the limits set by the finite and restricted character of our faculties, or destroy the endless unknown, which accompanies, like its shadow, the endless procession of phenomena. So far as I can venture to offer an opinion on such a matter, the purpose of our being in existence, the highest object that human beings can set before themselves, is not the pursuit of any such chimera as the annihilation of the unknown; but it is simply the unwearied endeavor to remove its boundaries a little farther from our little sphere of action.

I wonder if any historian would for a moment admit the objection that it is preposterous to trouble ourselves about the history of the Roman Empire because we do not know anything positive about the origin and first building of the city of Rome! Would it be a fair objection to urge respecting the sublime discoveries of a Newton, or a Kepler, those great philosophers whose discoveries have been of the profoundest benefit and service to all men—to say to them—"After all that you have told us as to how the planets revolve, and how they are maintained in their orbits, you cannot tell us what is the cause of the origin of the sun, moon, and stars. So what is the use of what you have done?" Yet these objections would not be one whit more preposterous than the objections which have been made to the "Origin of Species." Mr. Darwin, then, had a perfect right to limit his inquiry as he pleased, and the only question for us—the inquiry being so limited—is to ascertain whether the method of his inquiry is sound or unsound; whether he has obeyed the canons which must guide and govern all investigation, or whether he has broken them; and it was because our inquiry this evening is essentially limited to that question that I spent a good deal of time in a former lecture (which, perhaps, some of you thought might have been better employed) in endeavoring to illustrate the method and nature of scientific inquiry in general. We shall now have to put in practice the principles that I then laid down.

I stated to you in substance, if not in words, that wherever there are complex masses of phenomena to be inquired into, whether they be phenomena of the affairs of daily life, or whether they belong to the more abstruse and difficult problems laid before the philosopher, our course of proceeding in unravelling that complex chain of phenomena with a view to get at its cause is always the same; in all cases we must invent a hypothesis; we must place before ourselves some more or less likely supposition respecting that cause; and then, having assumed a hypothesis, having supposed a cause for the phenomena in question, we must endeavor, on the one hand, to demonstrate our hypothesis, or, on the other, to upset and reject it altogether, by testing it in three ways. We must, in the first place, be prepared to prove that the supposed causes of the phenomena exist in nature; that they are what the logicians call *vera causæ*—true causes; in the next place, we should be prepared to show that the assumed causes of the phenomena are competent to produce such phenomena as those which we wish to explain by them; and, in the last place, we ought to be able to show that no other known causes are competent to produce these phenomena. If we can succeed in satisfying these three conditions,

we shall have demonstrated our hypothesis; or rather, I ought to say, we shall have proved it as far as certainty is possible for us; for, after all, there is no one of our surest convictions which may not be upset, or at any rate modified, by a further accession of knowledge. It was because it satisfied these conditions that we accepted the hypothesis as to the disappearance of the tea-pot and spoons in the case I supposed in a previous lecture; we found that our hypothesis on that subject was tenable and valid, because the supposed cause existed in nature, because it was competent to account for the phenomena, and because no other known cause was competent to account for them; and it is upon similar grounds that any hypothesis you choose to name is accepted in science as tenable and valid.

What is Mr. Darwin's hypothesis? As I apprehend it—for I have put it into a shape more convenient for common purposes that I could find *verbatim* in his book—as I apprehend it, I say, it is, that all the phenomena of organic nature, past and present, result from, or are caused by, the inter-action of those properties of organic matter which we have called Atavism and Variability, with the Conditions of Existence; or, in other words—given the existence of organic matter, its tendency to transmit its properties, and its tendency occasionally to vary; and, lastly, given the conditions of existence by which organic matter is surrounded—that these put together are the causes of the present and of the past conditions of Organic Nature.

Such is the hypothesis as I understand it. Now let us see how it will stand the various tests which I laid down just now. In the first place, do these supposed causes of the phenomena exist in nature? Is it the fact that in nature these properties of organic matter—atavism and variability—and those phenomena which we have called the conditions of existence—is it true that they exist? Well, of course, if they do not exist, all that I have told you in the last three or four lectures must be incorrect, because I have been attempting to prove that they do exist, and I take it that there is abundant evidence that they do exist; so far, therefore, the hypothesis does not break down.

But in the next place comes a much more difficult inquiry: Are the causes indicated competent to give rise to the phenomena of organic nature? I suspect that this is indubitable to a certain extent. It is demonstrable, I think, as I have endeavored to show you, that they are perfectly competent to give rise to all the phenomena which are exhibited by races in nature. Furthermore, I believe that they are quite competent to account for all that we may call purely structural phenomena which are exhibited by species in nature. On that point also I have already enlarged somewhat. Again, I think that the causes assumed are competent to account for most of the physiological characteristics of species, and I not only think that they are competent to account for them, but I think that they account for many things which otherwise remain wholly unaccountable and inexplicable, and I may say incomprehensible. For a full exposition of the grounds on which this conviction is based, I must refer you to Mr. Darwin's work; all that I can do now is to illustrate what I have said by two or three cases taken almost at random.

I drew your attention, on a previous evening, to the facts which are embodied in our systems of classification, which are the results of the examination and comparison of the different members of the animal kingdom one with another. I mentioned that the whole of the animal kingdom is divisible into five sub-kingdoms; that each of these sub-kingdoms is again divisible into provinces; that each province may be divided into classes, and the classes into the successively smaller groups, orders, families, genera, and species.

Now, in each of these groups, the resemblance in structure among the members of the group is closer in proportion as the group is smaller. Thus, a man and a worm are members of the animal kingdom in virtue of certain apparently slight though really fundamental resemblances which they present. But a man and a fish are members of the same sub-kingdom *Vertebrata*, because they are much more like one another than either of them is to a worm, or a snail, or any member of the other sub-kingdoms. For similar reasons men and horses are arranged as members of the same class, *Mammalia*; men and apes as members of the same order, *Primates*; and if there were any animals more like men than they were like any of the apes, and yet different from men in important and constant particulars of their organization, we should rank them as members of the same family, or of the same genus, but as of distinct species.

That it is possible to arrange all the varied forms of animals into groups, having this sort of singular subordination one to the other, is a very remarkable circumstance; but, as Mr. Darwin remarks, this is a result which is quite to be expected, if the principles which he lays down be correct. Take the case of the races which are known to be produced by the operation of atavism and variability, and the conditions of existence which check and modify these tendencies. Take the case of the pigeons that I brought before you; there it was shown that they might be all classed as belonging to some one of five principal divisions, and that within these divisions other subordinate groups might be formed. The members of these groups are related to one another in just the same way as the genera of a family, and the groups themselves as the families of an order, or the orders of a class, while all have the same sort of structural relations with the wild rock pigeon, as the members of any great natural group have with a real or imaginary typical form. Now we know that all varieties of pigeons of every kind have arisen by a

process of selective breeding from a common stock, the rock pigeon; hence you see that if all species of animals have proceeded from some common stock, the general character of their structural relations, and of our systems of classification, which express those relations, would be just what we find them to be. In other words, the hypothetical cause is, so far, competent to produce effects similar to those of the real cause.

Take, again, another set of very remarkable facts; the existence of what are called rudimentary organs, organs for which we find no obvious use in the particular animal economy in which they are found, and yet which are there.

Such are the splint-like bones in the leg of the horse, which I here show you, and which correspond with bones which belong to certain toes and fingers in the human hand and foot. In the horse you see they are quite rudimentary, and bear neither toes nor fingers; so that the horse has only one "finger" in his fore-foot and one "toe" in his hind-foot. But it is a very curious thing that the animals closely allied to the horse show more toes than he, as the rhinoceros, for instance; he has these extra toes well formed, and anatomical facts show very clearly that he is very closely related to the horse indeed. So we may say that animals, in an anatomical sense nearly related to the horse, have those parts which are rudimentary in him fully developed.

Again, the sheep and the cow have no cutting-teeth, but only a hard pad in the upper jaw. That is the common characteristic of ruminants in general. But the calf has in its upper jaw some rudiments of teeth which never are developed, and never play the part of teeth at all. Well, if you go back in time, you find some of the older, now extinct, allies of the ruminants have well-developed teeth in their upper jaws; and at the present day the pig (which is in structure closely connected with ruminants) has well-developed teeth in its upper jaws; so that here is another instance of organs well developed and very useful, in one animal, represented by rudimentary organs, for which we can discover no purpose whatsoever, in another closely allied animal. The whalebone whale, again, has horny "whalebone" plates in its mouth, and no teeth; but the young fœtal whale, before it is born, has teeth in its jaws; they, however, are never used, and they never come to anything. But other members of the group to which the whale belongs have well-developed teeth in both jaws.

Upon any hypothesis of special creation, facts of this kind appear to me to be entirely unaccountable and inexplicable, but they cease to be so if you accept Mr. Darwin's hypothesis, and see reason for believing that the whalebone whale and the whale with teeth in its mouth both sprang from a whale that had teeth, and that the teeth of the fœtal whale are merely remnants—recollections, if we may so say—of the extinct whale. So in the case of the horse and the rhinoceros; suppose that both have descended by modification from some earlier form which had the normal number of toes, and the persistence of the rudimentary bones which no longer support toes in the horse becomes comprehensible.

In the language that we speak in England, and in the language of the Greeks, there are identical verbal roots, or elements, entering into the composition of words. The fact remains unintelligible so long as we suppose English and Greek to be independently created tongues; but when it is shown that both languages are descended from one original, the Sanscrit, we give an explanation of that resemblance. In the same way the existence of identical structural roots, if I may so term them, entering into the composition of widely different animals, is striking evidence in favor of the descent of those animals from a common original.

To turn to another kind of illustration: If you regard the whole series of stratified rocks—that enormous thickness of sixty or seventy thousand feet that I have mentioned before, constituting the only record we have of a most prodigious lapse of time, that time being, in all probability, but a fraction of that of which we have no record—if you observe in these successive strata of rocks successive groups of animals arising and dying out, a constant succession, giving you the same kind of impression, as you travel from one group of strata to another, as you would have in travelling from one country to another; when you find this constant succession of forms, their traces obliterated except to the man of science—when you look at this wonderful history, and ask what it means, it is only a paltering with words if you are offered the reply, "They were so created."

But if, on the other hand, you look on all forms of organized beings as the results of the gradual modification of a primitive type, the facts receive a meaning, and you see that these older conditions are the necessary predecessors of the present. Viewed in this light, the facts of palæontology receive a meaning; upon any other hypothesis I am unable to see, in the slightest degree, what knowledge or signification we are to draw out of them. Again note, as bearing upon the same point, the singular likeness which obtains between the successive faunæ and floræ, whose remains are preserved on the rocks: you never find any great and enormous difference between the immediately successive faunæ and floræ unless you have reason to believe there has also been a great lapse of time or a great change of conditions. The animals, for instance, of the newest tertiary rocks, in any part of the world, are always, and without exception, found to be closely allied with those which now live in that part of the world. For example, in Europe, Asia, and Africa the large mammals are at present rhinoceroses, hippopotamuses, elephants, lions, tigers, oxen,

horses, etc.; and if you examine the newest tertiary deposits, which contain the animals and plants which immediately preceded those which now exist in the same country, you do not find gigantic specimens of ant-eaters and kangaroos, but you find rhinoceroses, elephants, lions, tigers, etc., of different species to those now living, but still their close allies. If you turn to South America, where, at the present day, we have great sloths and armadilloes and creatures of that kind, what do you find in the newest tertiaries? You find the great sloth-like creature, the *Megatherium*, and the great armadillo, the *Glyptodon*, and so on. And if you go to Australia you find the same law holds good, namely, that that condition of organic nature which has preceded the one which now exists presents differences, perhaps, of species, and of genera, but that the great types of organic structure are the same as those which now flourish.

What meaning has this fact upon any other hypothesis or supposition than one of successive modification? But if the population of the world, in any age, is the result of the gradual modification of the forms which peopled it in the preceding age—if that has been the case, it is intelligible enough; because we may expect that the creature that results from the modification of an elephantine mammal shall be something like an elephant, and the creature which is produced by the modification of an armadillo-like mammal shall be like an armadillo. Upon that supposition, I say, the facts are intelligible; upon any other, that I am aware of, they are not.

So far, the facts of palæontology are consistent with almost any form of the doctrine of progressive modification; they would not be absolutely inconsistent with the wild speculations of De Maillet, or with the less objectionable hypothesis of Lamarck. But Mr. Darwin's views have one peculiar merit, and that is, that they are perfectly consistent with an array of facts which are utterly inconsistent with and fatal to any other hypothesis of progressive modification which has yet been advanced. It is one remarkable peculiarity of Mr. Darwin's hypothesis that it involves no necessary progression or incessant modification, and that it is perfectly consistent with the persistence for any length of time of a given primitive stock, contemporaneously with its modifications. To return to the case of the domestic breeds of pigeons, for example; you have the dove-cot pigeon, which closely resembles the rock pigeon, from which they all started, existing at the same time with the others. And if species are developed in the same way in nature, a primitive stock and its modifications may occasionally all find the conditions fitted for their existence; and though they come into competition, to a certain extent, with one another, the derivative species may not necessarily extirpate the primitive one, or *vice versa*.

Now palæontology shows us many facts which are perfectly harmonious with these observed effects of the process by which Mr. Darwin supposes species to have originated, but which appear to me to be totally inconsistent with any other hypothesis which has been proposed. There are some groups of animals and plants in the fossil world which have been said to belong to "persistent types," because they have persisted, with very little change indeed, through a very great range of time, while everything about them has changed largely. There are families of fishes whose type of construction has persisted all the way from the carboniferous rock right up to the cretaceous; and others which have lasted through almost the whole range of the secondary rocks, and from the lias to the older tertiaries. It is something stupendous this—to consider a genus lasting without essential modifications through all this enormous lapse of time while almost everything else was changed and modified.

Thus I have no doubt that Mr. Darwin's hypothesis will be found competent to explain the majority of the phenomena exhibited by species in nature; but in an earlier lecture I spoke cautiously with respect to its power of explaining all the physiological peculiarities of species.

There is, in fact, one set of these peculiarities which the theory of selective modification, as it stands at present, is not wholly competent to explain, and that is the group of phenomena which I mentioned to you under the name of Hybridism, and which I explained to consist in the sterility of the offspring of certain species when crossed one with another. It matters not one whit whether this sterility is universal, or whether it exists only in a single case. Every hypothesis is bound to explain, or, at any rate, not be inconsistent with, the whole of the facts which it professes to account for; and if there is a single one of these facts which can be shown to be inconsistent with (I do not merely mean inexplicable by, but contrary to) the hypothesis, the hypothesis falls to the ground; it is worth nothing. One fact with which it is positively inconsistent is worth as much, and as powerful in negativing the hypothesis, as five hundred. If I am right in thus defining the obligations of a hypothesis, Mr. Darwin, in order to place his views beyond the reach of all possible assault, ought to be able to demonstrate the possibility of developing from a particular stock by selective breeding two forms, which should either be unable to cross one with another, or whose cross-bred offspring should be infertile with one another.

For, you see, if you have not done that you have not strictly fulfilled all the conditions of the problem; you have not shown that you can produce, by the cause assumed, all the phenomena which you have in nature. Here are the phenomena of hybridism staring you in the face, and you cannot say, "I can, by selective modification, produce these same results." Now it is admitted on all hands that, at present, so far as experiments have gone, it has not been found pos-

sible to produce this complete physiological divergence by selective breeding. I stated this very clearly before, and I now refer to the point, because, if it could be proved, not only that this *has* not been done, but that it *cannot* be done; if it could be demonstrated that it is impossible to breed selectively, from any stock, a form which shall not breed with another, produced from the same stock; and if we were shown that this must be the necessary and inevitable result of all experiments, I hold that Mr. Darwin's hypothesis would be utterly shattered.

But has this been done? or what is really the state of the case? It is simply that, so far as we have gone yet with our breeding, we have not produced from a common stock two breeds which are not more or less fertile with one another.

I do not know that there is a single fact which would justify any one in saying that any degree of sterility has been observed between breeds absolutely known to have been produced by selective breeding from a common stock. On the other hand, I do not know that there is a single fact which can justify any one in asserting that such sterility cannot be produced by proper experimentation. For my own part, I see every reason to believe that it may, and will be so produced. For, as Mr. Darwin has very properly urged, when we consider the phenomena of sterility we find they are most capricious; we do not know what it is that the sterility depends on. There are some animals which will not breed in captivity; whether it arises from the simple fact of their being shut up and deprived of their liberty, or not, we do not know; but they certainly will not breed. What an astounding thing this is, to find one of the most important of all functions annihilated by mere imprisonment!

So, again, there are cases known of animals which have been thought by naturalists to be undoubted species, which have yielded fertile hybrids; while there are other species which present what everybody believes to be varieties* which are more or less infertile with one another. There are other cases which are truly extraordinary; there is one, for example, which has been carefully examined—of two kinds of sea-weed, of which the male element of the one, which we may call A, fertilizes the female element of the other, B, while the male element of B will not fertilize the female element of A; so that, while the former experiment seems to show us that they are *varieties*, the latter leads to the conviction that they are *species*.

When we see how capricious and uncertain this sterility is, how unknown the conditions on which it depends, I say that we have no right to affirm that those conditions will not be better understood by and by, and we have no ground for supposing that we may not be able to experiment so as to obtain that crucial result which I mentioned just now. So that though Mr. Darwin's hypothesis does not completely extricate us from this difficulty at present, we have not the least right to say it will not do so.

There is a wide gulf between the thing you cannot explain and the thing that upsets you altogether. There is hardly any hypothesis in this world which has not some fact in connection with it which has not been explained; but that is a very different affair to a fact that entirely opposes your hypothesis; in this case all you can say is that your hypothesis is in the same position as a good many others.

Now, as to the third test, that there are no other causes competent to explain the phenomena, I explained to you that one should be able to say of a hypothesis that no other known causes than those supposed by it are competent to give rise to the phenomena. Here, I think, Mr. Darwin's view is pretty strong. I really believe that the alternative is either Darwinism or nothing, for I do not know of any rational conception or theory of the organic universe which has any scientific position at all beside Mr. Darwin's. I do not know of any proposition that has been put before us with the intention of explaining the phenomena of organic nature, which has in its favor a thousandth part of the evidence which may be adduced in favor of Mr. Darwin's views. Whatever may be the objections to his views, certainly all others are absolutely out of court.

Take the Lamarckian hypothesis, for example. Lamarck was a great naturalist, and to a certain extent went the right way to work; he argued from what was undoubtedly a true cause of some of the phenomena of organic nature. He said it is a matter of experience that an animal may be modified more or less in consequence of its desires and consequent actions. Thus, if a man exercise himself as a blacksmith, his arms will become strong and muscular; such organic modification is a result of this particular action and exercise. Lamarck thought that by a very simple supposition based on this truth he could explain the origin of the various animal species; he said, for example, that the short-legged birds which live on fish had been converted into the long-legged waders by desiring to get the fish without wetting their feet, and so stretching their legs more and more through successive generations. If Lamarck could have shown experimentally that even races of animals could be produced in this way, there might have been some ground for his speculations. But he could show nothing of the kind, and his hypothesis has pretty well dropped into oblivion, as it deserved to do. I said in an earlier lecture that there are hypotheses and hypotheses, and when people tell you that Mr. Darwin's strongly-based hypothesis is nothing but a mere modification of Lamarck's, you will know what to think of their capacity for forming a judgment on this subject.

But you must recollect that when I say I

* And as I conceive with very good reason; but if any objector urges that we cannot prove that they have been produced by artificial or natural selection, the objection must be admitted, ultra-sceptical as it is. But in science scepticism is a duty.

think it is either Mr. Darwin's hypothesis or nothing, that either we must take his view or look upon the whole of organic nature as an enigma, the meaning of which is wholly hidden from us, you must understand that I mean that I accept it provisionally, in exactly the same way as I accept any other hypothesis. Men of science do not pledge themselves to creeds; they are bound by articles of no sort; there is not a single belief that it is not a bounden duty with them to hold with a light hand, and to part with it, cheerfully, the moment it is really proved to be contrary to any fact, great or small. And if in course of time I see good reasons for such a proceeding, I shall have no hesitation in coming before you and pointing out any change in my opinion without finding the slightest occasion to blush for so doing. So I say that we accept this view as we accept any other, so long as it will help us, and we feel bound to retain it only so long as it will serve our great purpose—the improvement of man's estate and the widening of his knowledge. The moment this, or any other conception, ceases to be useful for these purposes, away with it to the four winds; we care not what becomes of it!

But to say truth, although it has been my business to attend closely to the controversies roused by the publication of Mr. Darwin's book, I think that not one of the enormous mass of objections and obstacles which have been raised is of any great value, except that sterility case which I brought before you just now. All the rest are misunderstandings of some sort, arising either from prejudice or want of knowledge, or still more from want of patience and care in reading the work.

For you must recollect that it is not a book to be read with as much ease as its pleasant style may lead you to imagine. You spin through it as if it were a novel the first time you read it, and think you know all about it; the second time you read it you think you know rather less about it; and the third time you are amazed to find how little you have really apprehended its vast scope and objects. I can positively say that I never take it up without finding in it some new view, or light, or suggestion that I have not noticed before. That is the best characteristic of a thorough and profound book; and I believe this feature of the "Origin of Species" explains why so many persons have ventured to pass judgment and criticisms upon it which are by no means worth the paper they are written on.

Before concluding these lectures there is one point to which I must advert—though, as Mr. Darwin has said nothing about man in his boook, it concerns myself rather than him—for I have strongly maintained on sundry occasions that if Mr. Darwin's views are sound, they apply as much to man as to the lower mammals, seeing that it is perfectly demonstrable that the structural differences which separate man from the apes are not greater than those which separate some apes from others. There cannot be the slightest doubt in the world that the argument which applies to the improvement of the horse from an earlier stock, or of ape from ape, applies to the improvement of man from some simpler and lower stock than man. There is not a single faculty—functional or structural, moral, intellectual, or instinctive—there is no faculty whatever that is not capable of improvement; there is no faculty whatsoever which does not depend upon structure, and as structure tends to vary, it is capable of being improved.

Well, I have taken a good deal of pains at various times to prove this, and I have endeavored to meet the objections of those who maintain that the structural differences between man and the lower animals are of so vast a character and enormous extent that even if Mr. Darwin's views are correct, you cannot imagine this particular modification to take place. It is, in fact, easy matter to prove that, so far as structure is concerned, man differs to no greater extent from the animals which are immediately below him than these do from other members of the same order. Upon the other hand, there is no one who estimates more highly than I do the dignity of human nature, and the width of the gulf in intellectual and moral matters, which lies between man and the whole of the lower creation.

But I find this very argument brought forward vehemently by some. "You say that man has proceeded from a modification of some lower animal, and you take pains to prove that the structural differences which are said to exist in his brain do not exist at all, and you teach that all functions, intellectual, moral, and others, are the expression or the result, in the long run, of structures, and of the molecular forces which thy exert." It is quite true that I do so.

"Well, but," I am told at once, somewhat triumphantly, "you say in the same breath that there is a great moral and intellectual chasm between man and the lower animals. How is this possible when you declare that moral and intellectual characteristics depend on structure, and yet tell us that there is no such gulf between the structure of man and that of the lower animals?"

I think that objection is based upon a misconception of the real relations which exist between structure and function, between mechanism and work. Function is the expression of molecular forces and arrangements no doubt; but does it follow from this that variation in function so depends upon variation in structure that the former is always exactly proportioned to the latter? If there is no such relation, if the variation in function which follows on a variation in structure may be enormously greater than the variation of the structure, then, you see, the objection falls to the ground.

Take a couple of watches made by the same maker, and as completely alike as possible; set them upon the table, and the function of each—which is its rate of going—will be performed in the same manner, and you

shall be able to distinguish no difference between them; but let me take a pair of pincers, and if my hand is steady enough to do it, let me just lightly crush together the bearings of the balance-wheel, or force to a slightly different angle the teeth of the escapement of one of them, and of course you know the immediate result will be that the watch, so treated, from that moment will cease to go. But what proportion is there between the structural alteration and the functional result? Is it not perfectly obvious that the alteration is of the minutest kind, yet that, slight as it is, it has produced an infinite difference in the performance of the functions of these two instruments?

Well, now, apply that to the present question. What is it that constitutes and makes man what he is? What is it but his power of language—that language giving him the means of recording his experience—making every generation somewhat wiser than its predecessor, more in accordance with the established order of the universe?

What is it but this power of speech, of recording experience, which enables men to be men—looking before and after, and, in some dim sense, understanding the working of this wondrous universe—and which distinguishes man from the whole of the brute world? I say that this functional difference is vast, unfathomable, and truly infinite in its consequences; and I say at the same time that it may depend upon structural differences which shall be absolutely inappreciable to us with our present means of investigation. What is this very speech that we are talking about? I am speaking to you at this moment, but if you were to alter, in the minutest degree, the proportion of the nervous forces now active in the two nerves which supply the muscles of my glottis, I should become suddenly dumb. The voice is produced only so long as the vocal chords are parallel; and these are parallel only so long as certain muscles contract with exact equality; and that again depends on the equality of action of those two nerves I spoke of. So that a change of the minutest kind in the structure of one of these nerves, or in the structure of the part in which it originates, or of the supply of blood to that part, or of one of the muscles to which it is distributed, might render all of us dumb. But a race of dumb men, deprived of all communication with those who could speak, would be little indeed removed from the brutes. And the moral and intellectual difference between them and ourselves would be practically infinite, though the naturalist should not be able to find a single shadow of even specific structural difference.

But let me dismiss this question now, and, in conclusion, let me say that you may go away with it as my mature conviction that Mr. Darwin's work is the greatest contribution which has been made to biological science since the publication of the "Règne Animal" of Cuvier, and since that of the "*History of* Development" of Von Baer. I believe that if you strip it of its theoretical part it still remains one of the greatest encyclopædias of biological doctrine that any one man ever brought forth; and I believe that, if you take it as the embodiment of a hypothesis, it is destined to be the guide of biological and psychological speculation fo[r] the next three or four generations.

APPENDIX.

CRITICISMS ON DARWIN'S "ORIGIN OF SPECIES."

PROF. HUXLEY IN THE "NATURAL HISTORY REVIEW."

1. UEBER DIE DARWIN'SCHE SCHÖPFUNGSTHEORIE; EIN VORTRAG, VON A. KÖLLIKER. Leipzig, 1864.

2 EXAMINATION DU LIVRE DE M. DARWIN SUR L'ORIGINE DES ESPÈCES. Par P. FLOURENS. Paris, 1864.

IN the course of the present year (1864) several foreign commentaries upon Mr. Darwin's great work have made their appearance. Those who have perused that remarkable chapter of the "Antiquity of Man," in which Sir Charles Lyell draws a parallel between the development of species and that of languages, will be glad to hear that one of the most eminent philologers of Germany, Professor Schleicher, has, independently, published a most instructive and philosophical pamphlet (an excellent notice of which is to be found in the *Reader* for February 27th of this year) supporting similar views with all the weight of his special knowledge and established authority as a linguist. Professor Haeckel, to whom Schleicher addresses himself, previously took occasion, in his splendid monograph on the *Radiolaria*, to express his high appreciation of, and general concordance with, Mr. Darwin's views

But the most elaborate criticisms of the "Origin of Species" which have appeared are two works of very widely different merit, the one by Professor Kölliker, the well-known anatomist and histologist of Würzburg; the other by M. Flourens, Perpetual Secretary of the French Academy of Sciences.

Professor Kölliker's critical essay "Upon the Darwinian Theory" is, like all that proceeds from the pen of that thoughtful and accomplished writer, worthy of the most careful consideration. It comprises a brief but clear sketch of Darwin's views, followed by an enumeration of the leading difficulties in the way of their acceptance; difficulties which would appear to be insurmountable to Professor Kölliker, inasmuch as he proposes to replace Mr. Darwin's theory by one which he terms the "Theory of Heterogeneous Generation." We shall proceed to consider first the destructive, and secondly the constructive portion of the essay.

We regret to find ourselves compelled to dissent very widely from many of Professor Kölliker's remarks; and from none more thoroughly than from those in which he seeks

to define what we may term the philosophical position of Darwinism.

"Darwin," says Professor Kölliker, "is, in the fullest sense of the word, a teleologist. He says quite distinctly (First Edition, pp. 199, 200) that every particular in the structure of an animal has been created for its benefit, and he regards the whole series of animal forms only from this point of view."

And again:

"7. The teleological general conception adopted by Darwin is a mistaken one.

"Varieties arise irrespectively of the notion of purpose, or of utility, according to general laws of nature, and may be either useful, or hurtful, or indifferent.

"The assumption that an organism exists only on account of some definite end in view, and represents something more than the incorporation of a general idea, or law, implies a one-sided conception of the universe. Assuredly, every organ has, and every organism fulfils, its end, but its purpose is not the condition of its existence. Every organism is also sufficiently perfect for the purpose it serves, and in that, at least, it is useless to seek for a cause of its improvement."

It is singular how differently one and the same book will impress different minds. That which struck the present writer most forcibly on his first perusal of the "Origin of Species" was the conviction that teleology, as commonly understood, had received its death-blow at Mr. Darwin's hands. For the teleological argument runs thus: an organ or organism (A) is precisely fitted to perform a function or purpose (B); therefore it was specially constructed to perform that function. In Paley's famous illustration, the adaptation of all the parts of the watch to the function, or purpose, of showing the time, is held to be evidence that the watch was specially contrived to that end; on the ground that the only cause we know of, competent to produce such an effect as a watch which shall keep time, is a contriving intelligence adapting the means directly to that end.

Suppose, however, that any one had been able to show that the watch had not been made directly by any person, but that it was the result of the modification of another watch which kept time but poorly; and that this again had proceeded from a structure which could hardly be called a watch at all, seeing that it had no figures on the dial and the hands were rudimentary; and that going back and back in time we came at last to a revolving barrel as the earliest traceable rudiment of the whole fabric. And imagine that it had been possible to show that all these changes had resulted, first, from a tendency of the structure to vary indefinitely; and secondly, from something in the surrounding world which helped all variations in the direction of an accurate time-keeper, and checked all those in other directions; then it is obvious that the force of Paley's argument would be gone. For it would be demonstrated that an apparatus thoroughly well adapted to a particular purpose might be the result of a method of trial and error worked by unintelligent agents, as well as of the direct application of the means appropriate to that end, by an intelligent agent.

Now it appears to us that what we have here, for illustration's sake, supposed to be done with the watch is exactly what the establishment of Darwin's theory will do for the oranic world. For the notion that every organism has been created as it is and launched straight at a purpose, Mr. Darwin substitutes the conception of something which may fairly be termed a method of trial and error. Organisms vary incessantly; of these variations the few meet with surrounding conditions which suit them and thrive; the many are unsuited and become extinguished.

According to teleology, each organism is like a rifle bullet fired straight at a mark; according to Darwin, organisms are like grapeshot of which one hits something and the rest fall wide.

For the teleologist an organism exists because it was made for the conditions in which it is found; for the Darwinian an organism exists because, out of many of its kind, it is the only one which has been able to persist in the conditions in which it is found.

Teleology implies that the organs of every organism are perfect and cannot be improved; the Darwinian theory simply affirms that they work well enough to enable the organism to hold its own against such competitors as it has met with, but admits the possibility of indefinite improvement But an example may bring into clearer ligh the profound opposition between the ordinary teleological and the Darwinian conception.

Cats catch mice, small birds, and the like, very well. Teleology tells us that they do so because they were expressly constructed for so doing; that they are perfect mousing apparatuses, so perfect and so delicately adjusted that no one of their organs could be altered, without the change involving the alteration of all the rest. Darwinism affirms, on the contrary, that there was no express construction concerned in the matter; but that among the multitudinous variations of the feline stock, many of which died out from want of power to resist opposing influences, some, the cats, were better fitted to catch mice than others, whence they throve and persisted, in proportion to the advantage over their fellows thus offered to them.

Far from imagining that cats exist *in order* to catch mice well, Darwinism supposes that cats exist *because* they catch mice well—mousing being not the end, but the condition, of their existence. And if the cat-type has long persisted as we know it, the interpretation of the fact upon Darwinian principles would be, not that the cats have remained invariable, but that such varieties as have incessantly occurred have been, on the whole, less fitted to get on in the world than the existing stock.

If we apprehend the spirit of the "Origin of Species" rightly, then, nothing can be more entirely and absolutely opposed to teleology, as it is commonly understood, than the Darwinian theory. So far from being a "teleologist in the fullest sense of the word," we should deny that he is a teleologist in the ordinary sense at all; and we should say that, apart from his merits as a naturalist, he has rendered a most remarkable service to philosophical thought by enabling the student of nature to recognize, to their fullest extent, those adaptations to purpose which are so striking in the organic world, and which teleology has done good service in keeping before our minds, without being false to the fundamental principles of a scientific conception of the universe. The apparently diverging teachings of the teleologist and of the morphologist are reconciled by the Darwinian hypothesis.

But leaving our own impressions of the "Origin of Species," and turning to those passages specially cited by Professor Kölliker, we cannot admit that they bear the interpretation he puts upon them. Darwin, if we read him rightly, does *not* affirm that every detail in the structure of an animal has been created for its benefit. His words are (p. 199):

"The foregoing remarks lead me to say a few words on the protest lately made by some naturalists against the utilitarian doctrine that every detail of structure has been produced for the good of its possessor. They believe that very many structures have been created for beauty in the eyes of man, or for mere variety. This doctrine, if true, would be absolutely fatal to my theory, yet I fully admit that many structures are of no direct use to their possessor."

And after sundry illustrations and qualifications, he concludes (p. 200):

"Hence every detail of structure in every living creature (making some little allowance for the direct action of physical conditions) may be viewed either as having been of special use to some ancestral form, or as being now of special use to the descendants of this form—either directly or indirectly, through the complex laws of growth."

But it is one thing to say, Darwinically, that every detail observed in an animal's structure is of use to it, or has been of use to its ancestors; and quite another to affirm, teleologically, that every detail of an animal's structure has been created for its benefit. On the former hypothesis, for example, the teeth of the fœtal *Balæna* have a meaning; on the latter none. So far as we are aware, there is not a phrase in the "Origin of Species," inconsistent with Professor Kölliker's position, that "varieties arise irrespectively of the notion of purpose, or of utility, according to general laws of nature, and may be either useful, or hurtful, or indifferent.

On the contrary, Mr. Darwin writes (Summary of Chap. V.):

"*Our ignorance* of the laws of variation is profound. Not in one case out of a hundred can we pretend to assign any reason why this or that part varies more or less from the same part in the parents. . . . The external conditions of life, as climate and food, etc., seem to have induced some slight modifications. Habit, in producing constitutional differences, and use, in strengthening, and disuse, in weakening and diminishing organs, seem to have been more potent in their effects."

And finally, as if to prevent all possible misconception, Mr. Darwin concludes his "Chapter on Variation" with these pregnant words:

"Whatever the cause may be of each slight difference in the offspring from their parents—and a cause for each must exist—it is the steady accumulation, through natural selection of such differences, when beneficial to the individual, that gives rise to all the more important modifications of structure, by which the innumerable beings on the face of the earth are enabled to struggle with each other, and the best adapted to survive."

We have dwelt at length upon this subject, because of its great general importance, and because we believe that Professor Kölliker's criticisms on this head are based upon a misapprehension of Mr. Darwin's views—substantially they appear to us to coincide with his own. The other objections which Professor Kölliker enumerates and discusses are the following:

"1. No transitional forms between existing species are known; and known varieties, whether selected or spontaneous, never go so far as to establish new species."

To this Professor Kölliker appears to attach some weight. He makes the suggestion that the short-faced tumbler pigeon may be a pathological product.

"2. No transitional forms of animals are met with among the organic remains of earlier epochs."

Upon this, Professor Kölliker remarks that the absence of transitional forms in the fossil world, though not necessarily fatal to Darwin's views, weakens his case.

"3. The struggle for existence does not take place."

To this objection, urged by Pelzeln, Kölliker, very justly, attaches no weight.

"4. A tendency of organisms to give rise to useful varieties, and a natural selection, do not exist.

"The varieties which are found arise in consequence of manifold external influences, and it is not obvious why they all, or partially, should be particularly useful. Each animal suffices for its own ends, is perfect of its kind, and needs no further development. Should, however, a variety be useful and even maintain itself, there is no obvious reason why it should change any further. The whole conception of the imperfection of organisms and the necessity of their becoming perfected is plainly the weakest side of Darwin's theory, and a *pis aller* (Nothbehelf) because Darwin could think of no other

principle by which to explain the metamorphoses which, as I also believe, have occurred."

Here again we must venture to dissent completely from Professor Kölliker's conception of Mr. Darwin's hypothesis. It appears to us to be one of the many peculiar merits of that hypothesis that it involves no belief in a necessary and continual progress of organisms.

Again, Mr. Darwin, if we read him aright, assumes no special tendency of organisms to give rise to useful varieties, and knows nothing of needs of development, or necessity of perfection. What he says is, in substance: All organisms vary. It is in the highest degree improbable that any given variety should have exactly the same relations to surrounding conditions as the parent stock. In that case it is either better fitted (when the variation may be called useful), or worse fitted, to cope with them. If better, it will tend to supplant the parent stock; if worse, it will tend to be extinguished by the parent stock.

If (as is hardly conceivable) the new variety is so perfectly adapted to the conditions that no improvement upon it is possible—it will persist, because, though it does not cease to vary, the varieties will be inferior to itself.

If, as is more probable, the new variety is by no means perfectly adapted to its conditions, but only fairly well adapted to them, it will persist, so long as none of the varieties which it throws off are better adapted than itself.

On the other hand, as soon as it varies in a useful way—*i.e.*, when the variation is such as to adapt it more perfectly to its conditions—the fresh variety will tend to supplant the former.

So far from a gradual progress toward perfection forming any necessary part of the Darwinian creed, it appears to us that it is perfectly consistent with indefinite persistence in one state, or with a gradual retrogression. Suppose, for example, a return of the glacial epoch and a spread of polar climatal conditions over the whole globe. The operation of natural selection under these circumstances would tend, on the whole, to the weeding out of the higher organisms and the cherishing of the lower forms of life. Cryptogamic vegetation would have the advantage over phanerogamic; *Hydrozoa* over corals; *Crustacea* over *Insecta*, and *Amphipoda* and *Isopoda* over the higher *Crustacea*; cetaceans and seals over the *Primates*; the civilization of the Esquimaux over that of the European.

"5. Pelzeln has also objected that if the later organisms have proceeded from the earlier, the whole developmental series, from the simplest to the highest, could not now exist; in such a case the simpler organisms must have disappeared."

To this Professor Kölliker replies, with perfect justice, that the conclusion drawn by Pelzln does not really follow from Darwin's premises, and that, if we take the facts of palæontology as they stand, they rather support than oppose Darwin's theory.

"6. Great weight must be attached to the objection brought forward by Huxley, otherwise a warm supporter of Darwin's hypothesis, that we know of no varieties which are sterile with one another, as is the rule among sharply distinguished animal forms.

"If Darwin is right, it must be demonstrated that forms may be produced by selection, which, like the present sharply distinguished animal forms, are infertile when coupled with one another, and this has not been done."

The weight of this objection is obvious; but our ignorance of the conditions of fertility and sterility, the want of carefully conducted experiments extending over long series of years, and the strange anomalies presented by the results of the cross-fertilization of many plants, should all, as Mr. Darwin has urged, be taken into account in considering it.

The seventh objection is that we have already discussed.

The eighth and last stands as follows:

"8. The developmental theory of Darwin is not needed to enable us to understand the regular harmonious progress of the complete series of organic forms from the simpler to the more perfect.

"The existence of general laws of nature explains this harmony, even if we assume that all beings have arisen separately and independent of one another. Darwin forgets that inorganic nature, in which there can be no thought of a genetic connection of forms, exhibits the same regular plan, the same harmony, as the organic world; and that, to cite only one example, there is as much a natural system of minerals as of plants and animals."

We do not feel quite sure that we seize Professor Kölliker's meaning here, but he appears to suggest that the observation of the general order and harmony which pervade inorganic nature would lead us to anticipate a similar order and harmony in the organic world. And this is no doubt true, but it by no means follows that the particular order and harmony observed among them should be that which we see. Surely the stripes of dun horses, and the teeth of the fœtal *Balæna*, are not explained by the "existence of general laws of nature." Mr. Darwin endeavors to explain the exact order of organic nature which exists; not the mere fact that there is some order.

And with regard to the existence of a natural system of minerals; the obvious reply is that there may be a natural classification of any objects—of stones on a sea-beach, or of works of art; a natural classification being simply an assemblage of objects in groups, so as to express their most important and fundamental resemblances and differences. No doubt Mr. Darwin believes that those resemblances and differences upon which our natural systems or classifications of animals

and plants are based are resemblances and differences which have been produced genetically, but we can discover no reason for supposing that he denies the existence of natural classifications of other kinds.

And, after all, is it quite so certain that a genetic relation may not underlie the classification of minerals? The inorganic world has not always been what we see it. It has certainly had its metamorphoses, and, very probably, a long "Entwickelungsgeschichte" out of a nebular blastema. Who knows how far that amount of likeness among sets of minerals, in virtue of which they are now grouped into families and orders, may not be the expression of the common conditions to which that particular patch of nebulous fog, which may have been constituted by their atoms, and of which they may be, in the strictest sense, the descendants, was subjected?

It will be obvious from what has preceded that we do not agree with Professor Kölliker in thinking the objections which he brings forward so weighty as to be fatal to Darwin's view. But even if the case were otherwise, we should be unable to accept the "Theory of Heterogeneous Generation" which is offered as a substitute. That theory is thus stated:

"The fundamental conception of this hypothesis is that, under the influence of a general law of development, the germs of organisms produce others different from themselves. This might happen (1) by the fecundated ova passing, in the course of their development, under particular circumstances, into higher forms; (2) by the primitive and later organisms producing other organisms without fecundation out of germs or eggs (parthenogenesis)."

In favor of this hypothesis, Professor Kölliker adduces the well-known facts of agamogenesis, or "alternate generation;" the extreme dissimilarity of the males and females of many animals; and of the males, females, and neuters of those insects which live in colonies: and he defines its relations to the Darwinian theory as follows:

"It is obvious that my hypothesis is apparently very similar to Darwin's, inasmuch as I also consider that the various forms of animals have proceeded directly from one another. My hypothesis of the creation of organisms by heterogeneous generation, however, is distinguished very essentially from Darwin's by the entire absence of the principle of useful variations and their natural selection; and my fundamental conception is this, that a great plan of development lies at the foundation of the origin of the whole organic world, impelling the simpler forms to more and more complex developments. How this law operates, what influences determine the development of the eggs and germs, and impel them to assume constantly new forms, I naturally cannot pretend to say; but I can at least adduce the great analogy of the alternation of generations. If a *Bipinnaria*, a *Brachialaria*, a *Pluteus*, is competent to produce the Echinoderm, which is so widely different from it; if a hydroid polype can produce the higher medusa; if the vermiform trematode 'nurse' can develop within itself the very unlike *Cercaria*, it will not appear impossible that the egg, or ciliated embryo, of a sponge, for once, under special conditions, might become a hydroid polype, or the embryo of a medusa, an Echinoderm."

It is obvious from these extracts that Professor Kölliker's hypothesisis is based upon the supposed existence of a close analogy between the phenomena of agamogenesis and the production of new species from pre-existing ones. But is the analogy a real one? We think that it is not, and, by the hypothesis, cannot be.

For what are the phenomena of agamogenesis, stated generally? An impregnated egg develops into an asexual form A; this gives rise, asexually, to a second form or forms, B, more or less different from A. B may multiply asexually again; in the simple cases, however, it does not, but, acquiring sexual characters, produces impregnated eggs from whence A once more arises.

No case of agamogenesis is known in which, *when A differs widely from B*, it is itself capable of sexual propagation. No case whatever is known in which the progeny of B, by sexual generation, is other than a reproduction of A.

But if this be a true statement of the nature of the process of agamogenesis, how can it enable us to comprehend the production of new species from already existing ones? Let us suppose hyenas to have preceded dogs, and to have produced the latter in this way. Then the hyena will represent A, and the dog B. The first difficulty that presents itself is that the hyena must be asexual or the process will be wholly without analogy in the world of agamogenesis. But passing over this difficulty, and supposing a male and female dog to be produced at the same time from the hyena stock, the progeny of the pair, if the analogy of the simpler kinds of agamogenesis is to be followed, should be a litter, not of puppies, but of young hyenas. For the agamogenetic series is always, as we have seen, A : B : A : B, etc.; whereas for the production of a new species the series must be A : B : B : B, etc. The production of new species, or genera, is the extreme permanent divergence from the primitive stock. All known agamogenetic processes, on the other hand, end in a complete return to the primitive stock. How then is the production of new species to be rendered intelligible by the analogy of agamogenesis?

The other alternative put by Professor Kölliker—the passage of fecundated ova in the course of their development into higher forms—would, if it occurred, be merely an extreme case of variation in the Darwinian sense, greater in degree than, but perfectly similar in kind to, that which occurred when the well-known Ancon ram was developed from an ordinary ewe's ovum. Indeed we

have always thought that Mr. Darwin has unnecessarily hampered himself by adhering so strictly to his favorite "Natura non facit saltum." We greatly suspect that she does make considerable jumps in the way of variation now and then, and that these saltations give rise to some of the gaps which appear to exist in the series of known forms.

Strongly and freely as we have ventured to disagree with Professor Kölliker, we have always done so with regret, and we trust without violating that respect which is due, not only to his scientific eminence and to the careful study which he has devoted to the subject, but to the perfect fairness of his argumentation, and the generous appreciation of the worth of Mr. Darwin's labors which he always displays. It would be satisfactory to be able to say as much for M. Flourens.

But the Perpetual Secretary of the French Academy of Sciences deals with Mr. Darwin as the first Napoleon would have treated an "Idéologue;" and while displaying a painful weakness of logic and shallowness of information, assumes a tone of authority which always touches upon the ludicrous, and sometimes passes the limits of good breeding.

For example (p. 56):

"M. Darwin continue: 'Aucune distinction absolue n'a été et ne peut être établie entre les espèces et les variétés.' Je vous ai déjà dit que vous vous trompiez; une distinction absolue sépare les variétés d'avec les espèces."

"*Je vous ai déjà dit*; moi M. le Secrétaire perpétuel de l'Académie des Sciences: et vous

"'Qui n'êtes rien,
Pas même Académicien

what do you mean by asserting the contrary?" Being devoid of the blessings of an academy in England, we are unaccustomed to see our ablest men treated in this fashion, even by a "Perpetual Secretary."

Or again, considering that if there is any one quality of Mr. Darwin's work to which friends and foes have alike borne witness, it is his candor and fairness in admitting and discussing objections, what is to be thought of M. Flourens's assertion, that

"M. Darwin ne cite que les auteurs qui partagent ses opinions" (p. 40).

Once more (p. 65).

"Enfin l'ouvrage de M. Darwin a paru. On ne peut qu'être frappé du talent de l'auteur. Mais que d'idées obscures, que d'idées fausses! Quel jargon métaphysique jeté mal à propos dans l'histoire naturelle, qui tombe dans le galimatias dès qu'elle sort des idées claires, des idées justes! Quel langage prétentieux et vide! Quelles personifications puériles et surannées! O lucidité! O solidité de l'esprit Français, que devenez-vous?"

"Obscure ideas," "metaphysical jargon," "pretentious and empty language," "puerile and superannuated personifications." Mr. Darwin has many and hot opponents on this side of the Channel and in Germany, but we do not recollect to have found precisely these sins in the long catalogue of those hitherto laid to his charge. It is worth while, therefore, to examine into these discoveries effected solely by the aid of the "lucidity and solidity" of the mind of M. Flourens.

According to M. Flourens, Mr. Darwin's great error is that he has personified nature (p. 10), and further that he has

"imagined a natural selection; he imagines afterward that this power of selecting (*pouvoir d'élire*) which he gives to Nature is similar to the power of man. These two suppositions admitted, nothing stops him; he plays with nature as he likes, and makes her do all he pleases" (p. 6).

And this is the way M. Flourens extinguishes natural selection:

"Voyons donc encore une fois, ce qu'il peut y avoir de fondé dans ce qu'on nomme *élection naturelle*.

"*L'élection naturelle* n'est sous un autre nom que la nature. Pour un être organisé, la nature n'est que l'organization, ni plus ni moins.

"Il faudra donc aussi personnifier *l'organisation*, et dire que *l'organisation* choisit *l'organisation*. *L'élection naturelle* est cette *forme substantielle* dont on jouait autrefois avec tant de facilité. Aristote disait que 'Si l'art de bâtir était dans le bois, cet art agirait comme la nature.' A la place de *l'art de bâtir* M. Darwin met *l'élection naturelle*, et c'est tout un: l'un n'est pas plus chimérique que l'autre" (p. 31).

And this is really all that M. Flourens can make of natural selection. We have given the original, in fear lest a translation should be regarded as a travesty; but with the original before the reader, we may try to analyze the passage. "For an organized being, Nature is only organization, neither more nor less."

Organized beings then have absolutely no relation to inorganic nature; a plant does not depend on soil or sunshine, climate, depth in the ocean, height above it; the quantity of saline matters in water have no influence upon animal life; the substitution of carbonic acid for oxygen in our atmosphere would hurt nobody! That these are absurdities no one should know better than M. Flourens; but they are logical deductions from the assertion just quoted, and form the further statement that natural selection means only that "organization chooses and selects organization."

For if it be once admitted (what no sane man denies) that the chances of life of any given organism are increased by certain conditions (A) and diminished by their opposites (B), then it is mathematically certain that any change of conditions in the direction of (A) will exercise a selective influence in favor of that organism, tending to its increase and multiplication, while any change in the direction of (B) will exercise a selective influence against that organism, tending to its de-

crease and extinction.

Or, on the other hand, conditions remaining the same, let a given organism vary (and no one doubts that they do vary) in two directions; into one form (*a*) better fitted to cope with these conditions than the original stock, and a second (*b*) less well adapted to them. Then it is no less certain that the conditions in question must exercise a selective influence in favor of (*a*) and against (*b*), so that (*a*) will tend to predominance, and (*b*) to extirpation.

That M. Flourens should be unable to perceive the logical necessity of these simple arguments which lie at the foundation of all Mr. Darwin's reasoning; that he should confound an irrefragible deduction from the observed relations of organisms to the conditions which lie around them, with a metaphysical "forme substantielle," or a chimerical personification of the powers of nature, would be incredible, were it not that other passages of his work leave no room for doubt upon the subject.

"On imagine une *élection naturelle* que, pour plus de ménagement, on me dit être *inconsciente*, sans s'apercevoir que le contresens littéral est précisément là: *élection inconsciente*" (p. 52).

"J'ai déjà dit ce qu'il fautpenser de *l'élection naturelle*. *Ou l'élection naturelle* n'est rien, ou c'est la nature: mais la nature douée *d'élection*, mais la nature personnifiée; dernière erreur du dernier siècle: Le xix ne fait plus de personnifications" (p. 53).

M. Flourens cannot imagine an unconscious selection—it is for him a contradiction in terms. Did M. Flourens ever visit one of the prettiest watering-places of "la belle France," the Baie d'Arcachon? If so, he will probably have passed through the district of the Landes, and will have had an opportunity of observing the formation of "dunes" on a grand scale. What are these "dunes?" The winds and waves of the Bay of Biscay have not much consciousness, and yet they have with great care "selected," from among an infinity of masses of silex of all shapes and sizes, which have been submitted to their action, all the grains of sand below a certain size, and have heaped them by themselves over a great area. This sand has been "unconsciously selected" from amid the gravel in which it first lay with as much precision as if man had "consciously selected" it by the aid of a sieve. Physical geology is full of such selections—of the picking out of the soft from the hard, of the soluble from the insoluble, of the fusible from the infusible, by natural agencies to which we are certainly not in the habit of ascribing consciousness.

But that which wind and sea are to a sandy beach, the sum of influences, which we term the "conditions of existence," is to living organisms. The weak are sifted out from the strong. A frosty night "selects" the hardy plants in a plantation from among the tender ones as effectually as if it were the wind, and they, the sand and pebbles, of our illustration; or, on the other hand, as if the intelligence of a gardener had been operative in cutting the weaker organisms down. The thistle, which has spread over the pampas, to the destruction of native plants, has been more effectually "selected" by the unconscious operation of natural conditions than if a thousand agriculturists had spent their time in sowing it.

It is one of Mr. Darwin's many great services to biological science that he has demonstrated the significance of these facts. He has shown that—given variation and given change of conditions—the inevitable result is the exercise of such an influence upon organisms that one is helped and another is impeded; one tends to predominate, another to disappear; and thus the living world bears within itself, and is surrounded by impulses toward incessant change.

But the truths just stated are as certain as any other physical laws, quite independently of the truth, or falsehood, of the hypothesis which Mr. Darwin has based upon them; and that M. Flourens, missing the substance and grasping at a shadow, should be blind to the admirable exposition of them, which Mr. Darwin has given, and see nothing there but a "dernière erreur du dernier siècle—a personification of nature—leads us indeed to cry with him: O lucidité! O solidité de l'esprit Français, que devenez-vous?

M. Flourens has, in fact, utterly failed to comprehend the first principles of the doctrine which he assails so rudely. His objections to details are of the old sort, so battered and hackneyed on this side of the Channel that not even a Quarterly Reviewer could be induced to pick them up for the purpose of pelting Mr. Darwin over again. We have Cuvier and the mummies; M. Roulin and the domesticated animals of America; the difficulties presented by hybridism and by palæontology; Darwinism a *rifacciamento* of De Maillet and Lamarck; Darwinism a system without a commencement, and its author bound to believe in M. Pouchet, etc., etc. How one knows it all by heart, and with what relief one reads at p. 65,

"Je laisse M. Darwin!"

But we cannot leave M. Flourens without calling our reader's attention to his wonderful tenth chapter, "De la Préexistence des Germes et de l'Epigénèse, which opens thus:

"Spontaneous generation is only a chimera. This point established, two hypotheses remain—that of *pre-existence* and that of *epigenesis*. The one of these hypotheses has as little foundation as the other (p. 163).

"The doctrine of *epigenesis* is derived from Harvey: following by ocular inspection the development of the new being in the Windsor does, he saw each part appear successively, and taking the moment of *appearance* for the moment of *formation* he imagined *epigenesis* (p. 165.)

On the contrary, says M. Flourens (p. 167): "The new being is formed at a stroke (*tout*

d'un coup), as a whole instantaneously; it is not formed part by part, and at different times. It is formed at once; it is formed at the single *individual* moment at which the conjunction of the male and female elements takes place."

It will be observed that M. Flourens uses language which cannot be mistaken. For him, the labors of Von Baer, of Rathke, of Coste, and their contemporaries and successors in Germany, France, and England, are non-existent; and, as Darwin "*imagina*" natural selection, so Harvey "*imagina*" that doctrine which gives him an even greater claim to the veneration of posterity than his better-known discovery of the circulation of the blood.

Language such as that we have quoted is, in fact, so preposterous, so utterly incompatible with anything but absolute ignorance of some of the best established facts, that we should have passed it over in silence had it not appeared to afford some clew to M. Flourens's unhesitating, *à priori*, repudiation of all forms of the doctrine of the progressive modification of living beings. He whose mind remains uninfluenced by an acquaintance with the phenomena of development must indeed lack one of the chief motives toward the endeavor to trace a genetic relation between the different existing forms of life. Those who are ignorant of geology find no difficulty in believing that the world was made as it is; and the shepherd, untutored in history, sees no reason to regard the green mounds which indicate the site of a Roman camp as aught but part and parcel of the primeval hillside. So M. Flourens, who believes that embryos are formed "tout d'un coup," naturally finds no difficulty in conceiving that species came into existence in the same way.

THE END.

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BY

THOMAS H. HUXLEY.

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BY

THOMAS H. HUXLEY, F.R.S., F.L.S.,

ON THE PHYSICAL BASIS OF LIFE.*

In order to make the title of this discourse generally intelligible, I have translated the term "Protoplasm," which is the scientific name of the substance of

* The substance of this paper was contained in a discourse which was delivered in Edinburgh on the evening of Sunday, the 8th of November, 1868—being the first of a series of Sunday evening addresses upon non-theological topics, instituted by the Rev. J. Cranbrook. Some phrases, which could possess only a transitory and local interest, have been omitted; instead of the newspaper report of the Archbishop of York's address, his Grace's subsequently-published pamphlet "On the Limits of Philosophical Inquiry" is quoted; and I have, here and there, endeavored to express my meaning more fully and clearly than I seem to have done in speaking—if I may judge by sundry criticisms upon what I am supposed to have said, which have appeared. But in substance, and, so far as my recollection serves, in form, what is here written corresponds with what was there said.

which I am about to speak, by the words "the physical basis of life." I suppose that, to many, the idea that there is such a thing as a physical basis, or matter, of life may be novel—so widely spread is the conception of life as a something which works through matter, but is independent of it; and even those who are aware that matter and life are inseparably connected, may not be prepared for the conclusion plainly suggested by the phrase, "*the* physical basis or matter of life," that there is some one kind of matter which is common to all living beings, and that their endless diversities are bound together by a physical, as well as an ideal, unity. In fact, when first apprehended, such a doctrine as this appears almost shocking to common sense.

What, truly, can seem to be more obviously different from one another, in faculty, in form, and in substance, than the various kinds of living beings? What community of faculty can there be between the brightly-colored lichen, which so nearly resembles a mere mineral incrustation of the bare rock on which it grows, and the painter, to whom it is instinct with beauty, or the botanist, whom it feeds with knowledge?

Again, think of the microscopic fungus—a mere infinitesimal ovoid particle, which finds space and duration enough to multiply into countless millions in the body of a living fly; and then of the wealth of foliage, the luxuriance of flower and fruit, which lies between this bald sketch of a plant and the giant pine of California, towering to the dimensions of a cathedral spire, or the Indian fig, which covers acres with its profound shadow, and endures while nations and empires come and go around its vast circumference. Or, turning to the other half of the world of life, picture to yourselves the great Finner whale, hugest of beasts that live, or have lived, disporting his eighty or ninety feet of bone, muscle, and blubber, with easy roll, among waves in which the stoutest ship that ever left dockyard would founder hopelessly; and contrast him with the invisible animalcules—mere gelatinous specks, multitudes of which could, in fact, dance upon the point of a needle with the same ease as the angels of the Schoolmen could, in imagination. With these images before your minds, you may well ask, what community of form or structure is there between the animalcule and the whale; or between the fungus and the fig-tree? And, *à fortiori*, between all four?

Finally, if we regard substance, or material composition, what hidden bond can connect the flower which a girl wears in her hair and the blood which courses through her youthful veins; or what is there in common between the dense and resisting mass of the oak, or the strong fabric of the tortoise, and those broad disks of glassy jelly which may be seen pulsating through the waters of a calm sea, but which drain away to mere films in the hand which raises them out of their element?

Such objections as these must, I think, arise in the mind of every one who ponders, for the first time, upon the conception of a single physical basis of life, underlying all the diversities of vital existence; but I propose to demonstrate to you that, notwithstanding these apparent difficulties, a threefold unity—namely, a unity of power or faculty, a unity of form, and a unity of substantial composition—does pervade the whole living world.

No very abstruse argumentation is needed, in the first place, to prove that the powers or faculties of all kinds of living matter, diverse as they may be in degree, are substantially similar in kind.

Goethe has condensed a survey of all the powers of mankind into the well-known epigram:

"Warum treibt sich das Volk so und schreit?
Es will sich ernähren,
Kinder zeugen, und die nähren so gut es vermag.
* * * * * *
Weiter bringt es kein Mensch, stell' er sich wie er auch will."

In physiological language this means, that all the multifarious and complicated activities of man are comprehensible under three categories. Either they are immediately directed toward the maintenance and development of the body, or they effect transitory changes in the relative positions of parts of the body, or they tend toward the continuance of the species. Even those manifestations of intellect, of feeling, and of will, which we rightly name the higher faculties, are

not excluded from this classification, inasmuch as to every one but the subject of them, they are known only as transitory changes in the relative positions of parts of the body. Speech, gesture, and every other form of human action are, in the long run, resolvable into muscular contraction, and muscular contraction is but a transitory change in the relative positions of the parts of a muscle. But the scheme which is large enough to embrace the activities of the highest form of life covers all those of the lower creatures. The lowest plant, or animalcule, feeds, grows, and reproduces its kind. In addition, all animals manifest those transitory changes of form which we class under irritability and contractility; and it is more than probable that, when the vegetable world is thoroughly explored, we shall find all plants in possession of the same powers, at one time or other of their existence.

I am not now alluding to such phenomena, at once rare and conspicuous, as those exhibited by the leaflets of the sensitive plant, or the stamens of the barberry, but to much more widely-spread, and, at the same time, more subtle and hidden, manifestations of vegetable contractility. You are doubtless aware that the common nettle owes its stinging property to the innumerable stiff and needle-like, though exquisitely delicate, hairs which cover its surface. Each stinging-needle tapers from a broad base to a slender summit, which, though rounded at the end, is of such microscopic fineness that it readily penetrates, and breaks off in, the skin. The whole hair consists of a very delicate outer case of wood, closely applied to the inner surface of which is a layer of semi-fluid matter, full of innumerable granules of extreme minuteness. This semi-fluid lining is protoplasm, which thus constitutes a kind of bag, full of a limpid liquid, and roughly corresponding in form with the interior of the hair which it fills. When viewed with a sufficiently high magnifying power, the protoplasmic layer of the nettle hair is seen to be in a condition of unceasing activity. Local contractions of the whole thickness of its substance pass slowly and gradually from point to point, and give rise to the appearance of progressive waves, just as the bending of successive stalks of corn by a breeze produces the apparent billows of a corn-field.

But, in addition to these movements, and independently of them, the granules are driven, in relatively rapid streams, through channels in the protoplasm which seem to have a considerable amount of persistence. Most commonly, the currents in adjacent parts of the protoplasm take similar directions; and thus, there is a general stream up one side of the hair and down the other. But this does not prevent the existence of partial currents which take different routes; and sometimes, trains of granules may be seen coursing swiftly in opposite directions, within a twenty-thousandth of an inch of one another; while, occasionally, opposite streams come into direct collision, and, after a longer or shorter struggle, one predominates. The cause of these currents seems to lie in contractions of the protoplasm which bounds the channels in which they flow, but which are so minute that the best microscopes show only their effects, and not themselves.

The spectacle afforded by the wonderful energies prisoned within the compass of the microscopic hair of a plant, which we commonly regard as a merely passive organism, is not easily forgotten by one who has watched its display, continued hour after hour, without pause or sign of weakening. The possible complexity of many other organic forms, seemingly as simple as the protoplasm of the nettle, dawns upon one; and the comparison of such a protoplasm to a body with an internal circulation, which has been put forward by an eminent physiologist, loses much of its startling character. Currents similar to those of the hairs of the nettle have been observed in a great multitude of very different plants, and weighty authorities have suggested that they probably occur, in more or less perfection, in all young vegetable cells. If such be the case, the wonderful noonday silence of a tropical forest is, after all, due only to the dulness of our hearing; and could our ears catch the murmur of these tiny maelstroms, as they whirl in the innumerable myriads of living cells which constitute each tree, we should be stunned, as with the roar of a great city.

Among the lower plants, it is the rule

rather than the exception, that contractility should be still more openly manifested at some periods of their existence. The protoplasm of *Algæ* and *Fungi* becomes, under many circumstances, partially or completely freed from its woody case, and exhibits movements of its whole mass, or is propelled by the contractility of one or more hair-like prolongations of its body, which are called vibratile cilia. And, so far as the conditions of the manifestation of the phenomena of contractility have yet been studied, they are the same for the plant as for the animal. Heat and electric shocks influence both, and in the same way, though it may be in different degrees. It is by no means my intention to suggest that there is no difference in faculty between the lowest plant and the highest, or between plants and animals. But the difference between the powers of the lowest plant or animal, and those of the highest, is one of degree, not of kind, and depends, as Milne Edwards long ago so well pointed out, upon the extent to which the principle of the division of labor is carried out in the living economy. In the lowest organism all parts are competent to perform all functions, and one and the same portion of protoplasm may successively take on the function of feeding, moving, or reproducing apparatus. In the highest, on the contrary, a great number of parts combine to perform each function, each part doing its allotted share of the work with great accuracy and efficiency, but being useless for any other purpose.

On the other hand, notwithstanding all the fundamental resemblances which exist between the powers of the protoplasm in plants and in animals, they present a striking difference (to which I shall advert more at length presently), in the fact that plants can manufacture fresh protoplasm out of mineral compounds, whereas animals are obliged to procure it ready made, and hence, in the long run, depend upon plants. Upon what condition this difference in the powers of the two great divisions of the world of life depends, nothing is at present known.

With such qualification as arises out of the last-mentioned fact, it may be truly said that the acts of all living things are fundamentally one. Is any such unity predicable of their forms? Let us seek in easily verified facts for a reply to this question. If a drop of blood be drawn by pricking one's finger, and viewed with proper precautions and under a sufficiently high microscopic power, there will be seen, among the innumerable multitude of little, circular, discoidal bodies, or corpuscles, which float in it and give it its color, a comparatively small number of colorless corpuscles, of somewhat larger size and very irregular shape. If the drop of blood be kept at the temperature of the body, these colorless corpuscles will be seen to exhibit a marvellous activity, changing their forms with great rapidity, drawing in and thrusting out prolongations of their substance, and creeping about as if they were independent organisms.

The substance which is thus active is a mass of protoplasm, and its activity differs in detail, rather than in principle, from that of the protoplasm of the nettle. Under sundry circumstances the corpuscle dies and becomes distended into a round mass, in the midst of which is seen a smaller spherical body, which existed, but was more or less hidden, in the living corpuscle, and is called its *nucleus*. Corpuscles of essentially similar structure are to be found in the skin, in the lining of the mouth, and scattered through the whole framework of the body. Nay, more; in the earliest condition of the human organism, in that state in which it has but just become distinguishable from the egg in which it arises, it is nothing but an aggregation of such corpuscles, and every organ of the body was once no more than such an aggregation.

Thus a nucleated mass of protoplasm turns out to be what may be termed the structural unit of the human body. As a matter of fact, the body, in its earliest state, is a mere multiple of such units; and, in its perfect condition, it is a multiple of such units, variously modified.

But does the formula which expresses the essential structural character of the highest animal cover all the rest, as the statement of its powers and faculties covered that of all others? Very nearly. Beast and fowl, reptile and fish, mollusk, worm, and polype, are all composed of structural units of the same character,

namely, masses of protoplasm with a nucleus. There are sundry very low animals, each of which, structurally, is a mere colorless blood-corpuscle, leading an independent life. But, at the very bottom of the animal scale, even this simplicity becomes simplified, and all the phenomena of life are manifested by a particle of protoplasm without a nucleus. Nor are such organisms insignificant by reason of their want of complexity. It is a fair question whether the protoplasm of those simplest forms of life, which people an immense extent of the bottom of the sea, would not outweigh that of all the higher living beings which inhabit the land put together. And in ancient times, no less than at the present day, such living beings as these have been the greatest of rock builders.

What has been said of the animal world is no less true of plants. Imbedded in the protoplasm at the broad or attached end of the nettle hair, there lies a spheroidal nucleus. Careful examination further proves that the whole substance of the nettle is made up of a repetition of such masses of nucleated protoplasm, each contained in a wooden case, which is modified in form, sometimes into a woody fibre, sometimes into a duct or spiral vessel, sometimes into a pollen grain, or an ovule. Traced back to its earliest state, the nettle arises as the man does, in a particle of nucleated protoplasm. And in the lowest plants, as in the lowest animals, a single mass of such protoplasm may constitute the whole plant, or the protoplasm may exist without a nucleus.

Under these circumstances, it may well be asked, how is one mass of non-nucleated protoplasm to be distinguished from another? why call one "plant" and the other "animal"?

The only reply is that, so far as form is concerned, plants and animals are not separable, and that, in many cases, it is a mere matter of convention whether we call a given organism an animal or a plant. There is a living body called *Æthalium septicum*, which appears upon decaying vegetable substances, and, in one of its forms, is common upon the surfaces of tan-pits. In this condition it is, to all intents and purposes, a fungus, and formerly was always regarded as such; but the remarkable investigations of De Bary have shown that, in another condition, the *Æthalium* is an actively locomotive creature, and takes in solid matters, upon which, apparently, it feeds, thus exhibiting the most characteristic feature of animality. Is this a plant; or is it an animal? Is it both; or is it neither? Some decide in favor of the last supposition, and establish an intermediate kingdom, a sort of biological No Man's Land for all these questionable forms. But, as it is admittedly impossible to draw any distinct boundary line between this no man's land and the vegetable world on the one hand, or the animal on the other, it appears to me that this proceeding merely doubles the difficulty which, before, was single.

Protoplasm, simple or nucleated, is the formal basis of all life. It is the clay of the potter; which, bake it and paint it as he will, remains clay, separated by artifice, and not by nature, from the commonest brick or sun-dried clod.

Thus it becomes clear that all living powers are cognate, and that all living forms are fundamentally of one character. The researches of the chemist have revealed a no less striking uniformity of material composition in living matter.

In perfect strictness, it is true that chemical investigation can tell us little or nothing, directly, of the composition of living matter, inasmuch as such matter must needs die in the act of analysis—and upon this very obvious ground, objections, which I confess seem to me to be somewhat frivolous, have been raised to the drawing of any conclusions whatever respecting the composition of actually living matter, from that of the dead matter of life, which alone is accessible to us. But objectors of this class do not seem to reflect that it is also, in strictness, true that we know nothing about the composition of any body whatever, as it is. The statement that a crystal of calc-spar consists of carbonate of lime, is quite true, if we only mean that, by appropriate processes, it may be resolved into carbonic acid and quicklime. If you pass the same carbonic acid over the very quicklime thus obtained, you will obtain carbonate of lime again; but it will not be calc-spar, nor anything like it. Can it, therefore, be said that

chemical analysis teaches nothing about the chemical composition of calc-spar? Such a statement would be absurd; but it is hardly more so than the talk one occasionally hears about the uselessness of applying the results of chemical analysis to the living bodies which have yielded them.

One fact, at any rate, is out of reach of such refinements, and this is, that all the forms of protoplasm which have yet been examined contain the four elements, carbon, hydrogen, oxygen, and nitrogen, in very complex union, and that they behave similarly toward several reagents. To this complex combination, the nature of which has never been determined with exactness, the name of Protein has been applied. And if we use this term with such caution as may properly arise out of our comparative ignorance of the things for which it stands, it may be truly said that all protoplasm is proteinaceous; or, as the white or albumen of an egg is one of the commonest examples of a nearly pure proteine matter, we may say that all living matter is more or less albuminoid.

Perhaps it would not yet be safe to say that all forms of protoplasm are affected by the direct action of electric shocks; and yet the number of cases in which the contraction of protoplasm is shown to be effected by this agency increases every day.

Nor can it be affirmed with perfect confidence that all forms of protoplasm are liable to undergo that peculiar coagulation at a temperature of 40°–50° centigrade, which has been called "heat-stiffening," though Kühne's beautiful researches have proved this occurrence to take place in so many and such diverse living beings, that it is hardly rash to expect that the law holds good for all.

Enough has, perhaps, been said to prove the existence of a general uniformity in the character of the protoplasm, or physical basis, of life, in whatever group of living beings it may be studied. But it will be understood that this general uniformity by no means excludes any amount of special modifications of the fundamental substance. The mineral carbonate of lime assumes an immense diversity of characters, though no one doubts that, under all these protean changes, it is one and the same thing.

And now, what is the ultimate fate, and what the origin, of the matter of life?

Is it, as some of the older naturalists supposed, diffused throughout the universe in molecules, which are indestructible and unchangeable in themselves; but, in endless transmigration, unite in innumerable permutations, into the diversified forms of life we know? Or is the matter of life composed of ordinary matter, differing from it only in the manner which its atoms are aggregated? Is it built up of ordinary matter, and again resolved into ordinary matter when its work is done?

Modern science does not hesitate a moment between these alternatives. Physiology writes over the portals of life—

"Debemur morti nos nostraque,"

with a profounder meaning than the Roman poet attached to that melancholy line. Under whatever disguise it takes refuge, whether fungus or oak, worm or man, the living protoplasm not only ultimately dies and is resolved into its mineral and lifeless constituents, but is always dying, and, strange as the paradox may sound, could not live unless it died.

In the wonderful story of the "Peau de Chagrin," the hero becomes possessed of a magical wild ass' skin which yields him the means of gratifying all his wishes. But its surface represents the duration of the proprietor's life; and for every satisfied desire the skin shrinks in proportion to the intensity of fruition, until at length life and the last handbreadth of the *peau de chagrin* disappear with the gratification of a last wish.

Balzac's studies had led him over a wide range of thought and speculation, and his shadowing forth of physiological truth in this strange story may have been intentional. At any rate, the matter of life is a veritable *peau de chagrin*, and for every vital act it is somewhat the smaller. All work implies waste, and the work of life results, directly or indirectly, in the waste of protoplasm.

Every word uttered by a speaker costs him some physical loss; and, in the strictest sense, he burns that—ther may have light—so much eloquence, so much

of his body resolved into carbonic acid, water, and urea. It is clear that this process of expenditure cannot go on forever. But, happily, the protoplasmic *peau de chagrin* differs from Balzac's in its capacity of being repaired, and brought back to its full size, after every exertion.

For example, this present lecture, whatever its intellectual worth to you, has a certain physical value to me, which is, conceivably, expressible by the number of grains of protoplasm and other bodily substance wasted in maintaining my vital processes during its delivery. My *peau de chagrin* will be distinctly smaller at the end of the discourse than it was at the beginning. By and by, I shall probably have recourse to the substance commonly called mutton, for the purpose of stretching it back to its original size. Now this mutton was once the living protoplasm, more or less modified, of another animal—a sheep. As I shall eat it, it is the same matter altered, not only by death, but by exposure to sundry artificial operations in the process of cooking.

But these changes, whatever be their extent, have not rendered it incompetent to resume its old functions as matter of life. A singular inward laboratory, which I possess, will dissolve a certain portion of the modified protoplasm; the solution so formed will pass into my veins; and the subtle influences to which it will then be subjected will convert the dead protoplasm into living protoplasm, and transubstantiate sheep into man.

Nor is this all. If digestion were a thing to be trifled with, I might sup upon lobster, and the matter of life of the crustacean would undergo the same wonderful metamorphosis into humanity. And were I to return to my own place by sea, and undergo shipwreck, the crustacea might, and probably would, return the compliment, and demonstrate our common nature by turning my protoplasm into living lobster. Or, if nothing better were to be had, I might supply my wants with mere bread, and I should find the protoplasm of the wheat-plant to be convertible into man, with no more trouble than that of the sheep, and with far less, I fancy, than that of the lobster.

Hence it appears to be a matter of no great moment what animal, or what plant, I lay under contribution for protoplasm, and the fact speaks volumes for the general identity of that substance in all living beings. I share this catholicity of assimilation with other animals, all of which, so far as we know, could thrive equally well on the protoplasm of any of their fellows, or of any plant; but here the assimilative powers of the animal world cease. A solution of smelling-salts in water, with an infinitesimal proportion of some other saline matters, contains all the elementary bodies which enter into the composition of protoplasm; but, as I need hardly say, a hogshead of that fluid would not keep a hungry man from starving, nor would it save any animal whatever from a like fate. An animal cannot make protoplasm, but must take it ready-made from some other animal, or some plant—the animal's highest feat of constructive chemistry being to convert dead protoplasm into that living matter of life which is appropriate to itself.

Therefore, in seeking for the origin of protoplasm, we must eventually turn to the vegetable world. The fluid containing carbonic acid, water, and ammonia, which offers such a Barmecide feast to the animal, is a table richly spread to multitudes of plants; and, with a due supply of only such materials, many a plant will not only maintain itself in vigor, but grow and multiply until it has increased a million-fold, or a million million-fold, the quantity of protoplasm which it originally possessed; in this way building up the matter of life, to an indefinite extent, from the common matter of the universe.

Thus, the animal can only raise the complex substance of dead protoplasm to the higher power, as one may say, of living protoplasm; while the plant can raise the less complex substances—carbonic acid, water, and ammonia—to the same stage of living protoplasm, if not to the same level. But the plant also has its limitations. Some of the fungi, for example, appear to need higher compounds to start with; and no known plant can live upon the uncompounded elements of protoplasm. A plant supplied with pure carbon, hydrogen, oxygen, and nitrogen, phosphorus, sulphur, and the like, would as infallibly die as the animal in his bath of smelling-salts.

though it would be surrounded by all the constituents of protoplasm. Nor, indeed, need the process of simplification of vegetable food be carried so far as this, in order to arrive at the limit of the plant's thaumaturgy. Let water, carbonic acid, and all the other needful constituents be supplied with ammonia, and an ordinary plant will still be unable to manufacture protoplasm.

Thus the matter of life, so far as we know it (and we have no right to speculate on any other), breaks up, in consequence of that continual death which is the condition of its manifesting vitality, into carbonic acid, water, and ammonia, which certainly possess no properties but those of ordinary matter. And out of these same forms of ordinary matter, and from none which are simpler, the vegetable world builds up all the protoplasm which keeps the animal world a-going. Plants are the accumulators of the power which animals distribute and disperse.

But it will be observed, that the existence of the matter of life depends on the pre-existence of certain compounds; namely, carbonic acid, water, and ammonia. Withdraw any one of these three from the world, and all vital phenomena come to an end. They are related to the protoplasm of the plant, as the protoplasm of the plant is to that of the animal. Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these, carbon and oxygen unite, in certain proportions and under certain conditions, to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together, under certain conditions they give rise to the still more complex body, protoplasm, and this protoplasm exhibits the phenomena of life.

I see no break in this series of steps in molecular complication, and I am unable to understand why the language which is applicable to any one term of the series may not be used to any of the others. We think fit to call different kinds of matter carbon, oxygen, hydrogen, and nitrogen, and to speak of the various powers and activities of these substances as the properties of the matter of which they are composed.

When hydrogen and oxygen are mixed in a certain proportion, and an electric spark is passed through them, they disappear, and a quantity of water, equal in weight to the sum of their weights, appears in their place. There is not the slightest parity between the passive and active powers of the water and those of the oxygen and hydrogen which have given rise to it. At 32° Fahrenheit, and far below that temperature, oxygen and hydrogen are elastic gaseous bodies, whose particles tend to rush away from one another with great force. Water, at the same temperature, is a strong though brittle solid, whose particles tend to cohere into definite geometrical shapes, and sometimes build up frosty imitations of the most complex forms of vegetable foliage.

Nevertheless we call these, and many other strange phenomena, the properties of the water, and we do not hesitate to believe that, in some way or another, they result from the properties of the component elements of the water. We do not assume that a something called "aquosity" entered into and took possession of the oxide of hydrogen as soon as it was formed, and then guided the aqueous particles to their places in the facets of the crystal, or among the leaflets of the hoar-frost. On the contrary, we live in the hope and in the faith that, by the advance of molecular physics, we shall by and by be able to see our way as clearly from the constituents of water to the properties of water, as we are now able to deduce the operations of a watch from the form of its parts and the manner in which they are put together.

Is the case in any way changed when carbonic acid, water, and ammonia disappear, and in their place, under the influence of pre-existing living protoplasm, an equivalent weight of the matter of life makes its appearance?

It is true that there is no sort of parity between the properties of the components and the properties of the resultant, but neither was there in the case of the water. It is also true that what I have spoken of as the influence of pre-existing living matter is something quite unintelligible; but does anybody quite comprehend the *modus operandi* of an electric

spark, which traverses a mixture of oxygen and hydrogen ?

What justification is there, then, for the assumption of the existence in the living matter of a something which has no representative, or correlative, in the not living matter which gave rise to it ? What better philosophical status has "vitality" than "aquosity" ? And why should "vitality" hope for a better fate than the other "itys" which have disappeared since Martinus Scriblerus accounted for the operation of the meat-jack by its inherent "meat-roasting quality," and scorned the "materialism" of those who explained the turning of the spit by a certain mechanism worked by the draught of the chimney ?

If scientific language is to possess a definite and constant signification whenever it is employed, it seems to me that we are logically bound to apply to the protoplasm, or physical basis of life, the same conceptions as those which are held to be legitimate elsewhere. If the phenomena exhibited by water are its properties, so are those presented by protoplasm, living or dead, its properties.

If the properties of water may be properly said to result from the nature and disposition of its component molecules, I can find no intelligible ground for refusing to say that the properties of protoplasm result from the nature and disposition of its molecules.

But I bid you beware that, in accepting these conclusions, you are placing your feet on the first rung of a ladder which, in most people's estimation, is the reverse of Jacob's, and leads to the antipodes of heaven. It may seem a small thing to admit that the dull vital actions of a fungus or a foraminifer are the properties of their protoplasm, and are the direct results of the nature of the matter of which they are composed. But if, as I have endeavored to prove to you, their protoplasm is essentially identical with, and most readily converted into, that of any animal, I can discover no logical halting-place between the admission that such is the case, and the further concession that all vital action may, with equal propriety, be said to be the result of the molecular forces of the protoplasm which displays it. And if so, it must be true, in the same sense and to the same extent, that the thoughts to which I am now giving utterance, and your thoughts regarding them, are the expression of molecular changes in that matter of life which is the source of our other vital phenomena.

Past experience leads me to be tolerably certain that, when the propositions I have just placed before you are accessible to public comment and criticism, they will be condemned by many zealous persons, and perhaps by some few of the wise and thoughtful. I should not wonder if "gross and brutal materialism" were the mildest phrase applied to them in certain quarters. And, most undoubtedly, the terms of the propositions are distinctly materialistic. Nevertheless two things are certain : the one, that I hold the statements to be substantially true ; the other, that I, individually, am no materialist, but, on the contrary, believe materialism to involve grave philosophical error.

This union of materialistic terminology with the repudiation of materialistic philosophy I share with some of the most thoughtful men with whom I am acquainted. And, when I first undertook to deliver the present discourse, it appeared to me to be a fitting opportunity to explain how such a union is not only consistent with, but necessitated by, sound logic. I purposed to lead you through the territory of vital phenomena to the materialistic slough in which you find yourselves now plunged, and then to point out to you the sole path by which, in my judgment, extrication is possible.

An occurrence of which I was unaware until my arrival here last night renders this line of argument singularly opportune. I found in your papers the eloquent address "On the Limits of Philosophical Inquiry," which a distinguished prelate of the English Church delivered before the members of the Philosophical Institution on the previous day. My argument, also, turns upon this very point of the limits of philosophical inquiry ; and I cannot bring out my own views better than by contrasting them with those so plainly and, in the main, fairly stated by the Archbishop of York.

But I may be permitted to make a preliminary comment upon an occurrence

that greatly astonished me. Applying the name of the "New Philosophy" to that estimate of the limits of philosophical inquiry which I, in common with many other men of science, hold to be just, the archbishop opens his address by identifying this "New Philosophy" with the Positive Philosophy of M. Comte (of whom he speaks as its "founder"); and then proceeds to attack that philosopher and his doctrines vigorously.

Now, so far as I am concerned, the most reverend prelate might dialectically hew M. Comte in pieces, as a modern Agog, and I should not attempt to stay his hand. In so far as my study of what specially characterizes the Positive Philosophy has led me, I find therein little or nothing of any scientific value, and a great deal which is as thoroughly antagonistic to the very essence of science as anything in ultramontane Catholicism. In fact, M. Comte's philosophy in practice might be compendiously described as Catholicism *minus* Christianity.

But what has Comtism to do with the "New Philosophy," as the archbishop defines it in the following passage?

"Let me briefly remind you of the leading principles of this new philosophy.

"All knowledge is experience of facts acquired by the senses. The traditions of older philosophies have obscured our experience by mixing with it much that the senses cannot observe, and until these additions are discarded our knowledge is impure. Thus metaphysics tell us that one fact which we observe is a cause, and another is the effect of that cause; but, upon a rigid analysis, we find that our senses observe nothing of cause or effect: they observe, first, that one fact succeeds another, and, after some opportunity, that this fact has never failed to follow—that for cause and effect we should substitute invariable succession. An older philosophy teaches us to define an object by distinguishing its essential from its accidental qualities: but experience knows nothing of essential and accidental; she sees only that certain marks attach to an object, and, after many observations, that some of them attach invariably, while others may at times be absent. . . . As all knowledge is relative, the notion of anything being necessary must be banished with other traditions." *

There is much here that expresses the spirit of the "New Philosophy," if by that term be meant the spirit of modern science; but I cannot but marvel that the assembled wisdom and learning of Edinburgh should have uttered no sign of dissent, when Comte was declared to be the founder of these doctrines. No one will accuse Scotchmen of habitually forgetting their great countrymen; but it was enough to make David Hume turn in his grave, that here, almost within earshot of his house, an instructed audience should have listened, without a murmur, while his most characteristic doctrines were attributed to a French writer of fifty years later date, in whose dreary and verbose pages we miss alike the vigor of thought and the exquisite clearness of style of the man whom I make bold to term the most acute thinker of the eighteenth century—even though that century produced Kant.

But I did not come to Scotland to vindicate the honor of one of the greatest men she has ever produced. My business is to point out to you that the only way of escape out of the crass materialism in which we just now landed, is the adoption and strict working-out of the very principles which the archbishop holds up to reprobation.

Let us suppose that knowledge is absolute, and not relative, and therefore, that our conception of matter represents that which it really is. Let us suppose, further, that we do know more of cause and effect than a certain definite order of succession among facts, and that we have a knowledge of the necessity of that succession—and hence, of necessary laws—and I, for my part, do not see what escape there is from utter materialism and necessarianism. For it is obvious that our knowledge of what we call the material world is, to begin with, at least as certain and definite as that of the spiritual world, and that our acquaintance with law is of as old a date as our knowledge of spontaneity. Further, I take it to be demonstrable that it is utterly impossible to prove that anything whatever may not be the effect of a material and necessary cause, and that human logic is equally incompetent to prove that any act is really spontaneous. A really spontaneous act is one which, by the assumption, has no cause; and the attempt to prove such a negative as this is, on the face of the matter, absurd. And while it is thus

* "The Limits of Philosophical Inquiry," pp. 4 and 5.

a philosophical impossibility to demonstrate that any given phenomenon is not the effect of a material cause, any one who is acquainted with the history of science will admit, that its progress has, in all ages, meant, and now, more than ever, means, the extension of the province of what we call matter and causation, and the concomitant gradual banishment from all regions of human thought of what we call spirit and spontaneity.

I have endeavored, in the first part of this discourse, to give you a conception of the direction toward which modern physiology is tending; and I ask you, what is the difference between the conception of life as the product of a certain disposition of material molecules, and the old notion of an Archæus governing and directing blind matter within each living body, except this—that here, as elsewhere, matter and law have devoured spirit and spontaneity? And as surely as every future grows out of past and present, so will the physiology of the future gradually extend the realm of matter and law until it is co-extensive with knowledge, with feeling, and with action.

The consciousness of this great truth weighs like a nightmare, I believe, upon many of the best minds of these days. They watch what they conceive to be the progress of materialism, in such fear and powerless anger as a savage feels, when, during an eclipse, the great shadow creeps over the face of the sun. The advancing tide of matter threatens to drown their souls; the tightening grasp of law impedes their freedom; they are alarmed lest man's moral nature be debased by the increase of his wisdom.

If the "New Philosophy" be worthy of the reprobation with which it is visited, I confess their fears seem to me to be well founded. While, on the contrary, could David Hume be consulted, I think he would smile at their perplexities, and chide them for doing even as the heathen, and falling down in terror before the hideous idols their own hands have raised.

For, after all, what do we know of this terrible "matter," except as a name for the unknown and hypothetical cause of states of our own consciousness? And what do we know of that "spirit" over whose threatened extinction by matter a great lamentation is arising, like that which was heard at the death of Pan, except that it is also a name for an unknown and hypothetical cause, or condition, of states of consciousness? In other words, matter and spirit are but names for the imaginary substrata of groups of natural phenomena.

And what is the dire necessity and "iron" law under which men groan? Truly, most gratuitously invented bugbears. I suppose if there be an "iron" law, it is that of gravitation; and if there be a physical necessity, it is that a stone, unsupported, must fall to the ground. But what is all we really know, and can know, about the latter phenomenon? Simply, that, in all human experience, stones have fallen to the ground under these conditions; that we have not the smallest reason for believing that any stone so circumstanced will not fall to the ground; and that we have, on the contrary, every reason to believe that it will so fall. It is very convenient to indicate that all the conditions of belief have been fulfilled in this case, by calling the statement that unsupported stones will fall to the ground "a law of nature." But when, as commonly happens, we change *will* into *must*, we introduce an idea of necessity which most assuredly does not lie in the observed facts, and has no warranty that I can discover elsewhere. For my part, I utterly repudiate and anathematize the intruder. Fact I know; and law I know; but what is this necessity, save an empty shadow of my own mind's throwing?

But, if it is certain that we can have no knowledge of the nature of either matter or spirit, and that the notion of necessity is something illegitimately thrust into the perfectly legitimate conception of law, the materialistic position that there is nothing in the world but matter, force, and necessity, is as utterly devoid of justification as the most baseless of theological dogmas. The fundamental doctrines of materialism, like those of spiritualism, and most other "isms," lie outside "the limits of philosophical inquiry," and David Hume's great service to humanity is his irrefragable demonstration of what these limits are.

Hume called himself a sceptic, and therefore others cannot be blamed if they apply the same title to him; but that does not alter the fact that the name, with its existing implications, does him gross injustice.

If a man asks me what the politics of the inhabitants of the moon are, and I reply that I do not know; that neither I, nor any one else, have any means of knowing; and that, under these circumstances, I decline to trouble myself about the subject at all, I do not think he has any right to call me a sceptic. On the contrary, in replying thus, I conceive that I am simply honest and truthful, and show a proper regard for the economy of time. So Hume's strong and subtle intellect takes up a great many problems about which we are naturally curious, and shows us that they are essentially questions of lunar politics, in their essence incapable of being answered, and therefore not worth the attention of men who have work to do in the world. And he thus ends one of his essays:

"If we take in hand any volume of divinity, or school metaphysics, for instance, let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames; for it can contain nothing but sophistry and illusion." *

Permit me to enforce this most wise advice. Why trouble ourselves about matters of which, however important they may be, we do know nothing, and can know nothing? We live in a world which is full of misery and ignorance, and the plain duty of each and all of us is to try to make the little corner he can influence somewhat less miserable and somewhat less ignorant than it was before he entered it. To do this effectually it is necessary to be fully possessed of only two beliefs: the first, that the order of nature is ascertainable by our faculties to an extent which is practically unlimited; the second, that our volition counts for something as a condition of the course of events.

Each of these beliefs can be verified experimentally, as often as we like to try. Each, therefore, stands upon the strongest foundation upon which any belief can rest, and forms one of our highest truths. If we find that the ascertainment of the order of nature is facilitated by using one terminology, or one set of symbols, rather than another, it is our clear duty to use the former; and no harm can accrue, so long as we bear in mind that we are dealing merely with terms and symbols.

In itself it is of little moment whether we express the phenomena of matter in terms of spirit; or the phenomena of spirit, in terms of matter: matter may be regarded as a form of thought, thought may be regarded as a property of matter—each statement has a certain relative truth. But with a view to the progress of science, the materialistic terminology is in every way to be preferred. For it connects thought with the other phenomena of the universe, and suggests inquiry into the nature of those physical conditions, or concomitants of thought, which are more or less accessible to us, and a knowledge of which may, in future, help us to exercise the same kind of control over the world of thought as we already possess in respect of the material world; whereas, the alternative, or spiritualistic, terminology is utterly barren, and leads to nothing but obscurity and confusion of ideas.

Thus there can be little doubt, that the further science advances, the more extensively and consistently will all the phenomena of nature be represented by materialistic formulæ and symbols.

But the man of science, who, forgetting the limits of philosophical inquiry, slides from these formulæ and symbols into what is commonly understood by materialism, seems to me to place himself on a level with the mathematician, who should mistake the x's and y's with which he works his problems, for real entities—and with this further disadvantage, as compared with the mathematician, that the blunders of the latter are of no practical consequence, while the errors of systematic materialism may paralyze the energies and destroy the beauty of a life.

* Hume's essay "Of the Academical or Sceptical Philosophy," in the "Inquiry concerning the Human Understanding."

THE SCIENTIFIC ASPECTS OF POSITIVISM.*

It is now some sixteen or seventeen years since I became acquainted with the "Philosophie Positive," the "Discours sur l'Ensemble du Positivisme," and the "Politique Positive" of Auguste Comte. I was led to study these works partly by the allusions to them in Mr. Mill's "Logic," partly by the recommendation of a distinguished theologian, and partly by the urgency of a valued friend, the late Professor Henfrey, who looked upon M. Comte's bulky volumes as a mine of wisdom, and lent them to me that I might dig and be rich. After due perusal, I found myself in a position to echo my friend's words, though I may have laid more stress on the "mine" than on the "wisdom." For I found the veins of ore few and far between, and the rock so apt to run to mud, that one incurred the risk of being intellectually smothered in the working. Still, as I was glad to acknowledge, I did come to a nugget here and there; though not, so far as my experience went, in the discussions on the philosophy of the physical sciences, but in the chapters on speculative and practical sociology. In these there was indeed much to arouse the liveliest interest in one whose boat had broken away from the old moorings, and who had been content "to lay out an anchor by the stern" until daylight should break and the fog clear. Nothing could be more interesting to a student of biology than to see the study of the biological sciences laid down as an essential part of the prolegomena of a new view of social phenomena. Nothing could be more satisfactory to a worshipper of the severe truthfulness of science than the attempt to dispense with all beliefs, save such as could brave the light, and seek, rather than fear, criticism; while, to a lover of courage and outspokenness, nothing could be more touching than the placid announcement on the title-page of the "Discourse sur l'Ensemble du Positivisme," that its author proposed

"Réorganiser, sans Dieu ni roi,
Par le culte systématique de l'Humanité,"

the shattered frame of modern society.

In those days I knew my "Faust" pretty well, and, after reading this word of might, I was minded to chant the well-known stanzas of the "Geisterchor"—

"Weh! Weh!
Die schöne Welt,
Sie stürzt, sie zerfällt:
Wir tragen
Die Trümmern ins Nichts hinüber.
Mächtiger
Der Erdensöhne,
Prächtiger
Baue sie wieder;
In deinem Busen baue sie auf."

Great, however, was my perplexity, not to say disappointment, as I followed the progress of this "mighty son of earth" in his work of reconstruction. Undoubtedly "Dieu" disappeared, but the "Nouveau Grand-Etre Suprême," a gigantic fetish, turned out brand-new by M. Comte's own hands, reigned in his stead. "Roi" also was not heard of; but, in his place, I found a minutely-defined social organization, which, if it ever came into practice, would exert a despotic authority such as no sultan has rivalled, and no Puritan presbytery, in its palmiest days, could hope to excel. While as for the "culte systématique de l'Humanité," I, in my blindness, could not distinguish it from sheer popery, with M. Comte in the chair of St. Peter, and the names of most of the saints changed. To quote "Faust" again, I found myself saying with Gretchen,—

"Ungefähr sagt das der Pfarrer auch,
Nur mit ein bischen andern Worten."

Rightly or wrongly, this was the impression which, all those years ago, the study of M. Comte's works left on my mind, combined with the conviction, which I shall always be thankful to him for awakening in me, that the organization of society upon a new and purely scientific basis is not only practicable, but is the only political object much worth fighting for.

As I have said, that part of M. Comte's writings which deals with the philosophy of physical science appeared to me to possess singularly little value, and to show that he had but the most superficial, and merely second-hand, knowledge

* A reply to Mr. Congreve's attack upon the preceding paper. Published in the *Fortnightly Review*, 1869.

of most branches of what is usually understood by science. I do not mean by this merely to say that Comte was behind our present knowledge, or that he was unacquainted with the details of the science of his own day. No one could justly make such defects cause of complaint in a philosophical writer of the past generation. What struck me was his want of apprehension of the great features of science; his strange mistakes as to the merits of his scientific contemporaries; and his ludicrously erroneous notions about the part which some of the scientific doctrines current in his time were destined to play in the future. With these impressions in my mind, no one will be surprised if I acknowledge that, for these sixteen years, it has been a periodical source of irritation to me to find M. Comte put forward as a representative of scientific thought; and to observe that writers whose philosophy had its legitimate parent in Hume, or in themselves, were labelled "Comtists" or "Positivists" by public writers, even in spite of vehement protests to the contrary. It has cost Mr. Mill hard rubbings to get that label off; and I watch Mr. Spencer, as one regards a good man struggling with adversity, still engaged in eluding its adhesiveness, and ready to tear away skin and all, rather than let it stick. My own turn might come next; and therefore, when an eminent prelate the other day gave currency and authority to the popular confusion, I took an opportunity of incidentally revindicating Hume's property in the so-called "New Philosophy," and, at the same time, of repudiating Comtism on my own behalf.*

The few lines devoted to Comtism in my paper on the "Physical Basis of Life" were, in intention, strictly limited to these two purposes. But they seem to have given more umbrage than I intended they should, to the followers of M. Comte in this country, for some of whom, let me observe in passing, I entertain a most unfeigned respect; and Mr. Congreve's recent article gives expression to the displeasure which I have excited among the members of the Comtian body.

Mr. Congreve, in a peroration which seems especially intended to catch the attention of his readers, indignantly challenges me to admire M. Comte's life, "to deny that it has a marked character of grandeur about it;" and he uses some very strong language because I show no sign of veneration for his idol. I confess I do not care to occupy myself with the denigration of a man who, on the whole, deserves to be spoken of with respect. Therefore, I shall enter into no statement of the reasons which lead me unhesitatingly to accept Mr. Congreve's challenge, and to refuse to recognize anything which deserves the name of grandeur of character in M. Comte, unless it be his arrogance, which is undoubtedly sublime. All I have to observe is, that if Mr. Congreve is justified in saying that I speak with a tinge of contempt for his spiritual father, the reason for such coloring of my

* I am glad to observe that Mr. Congreve, in the criticism with which he has favored me in the number of the *Fortnightly Review* for April, 1869, does not venture to challenge the justice of the claim I made for Hume. He merely suggests that I have been wanting in candor in not mentioning Comte's high opinion of Hume. After mature reflection, I am unable to discern my fault. If I had suggested that Comte had borrowed from Hume without acknowledgment; or if, instead of trying to express my own sense of Hume's merits with the modesty which becomes a writer who has no authority in matters of philosophy, I had affirmed that no one had properly appreciated him, Mr. Congreve's remarks would apply: but as I did neither of these things, they appear to me to be irrelevant, if not unjustifiable. And even had it occurred to me to quote M. Comte's expressions about Hume, I do not know that I should have cited them, inasmuch as, on his own showing, M. Comte occasionally speaks very decidedly touching writers of whose works he has not read a line. Thus, in Tome VI. of the "Philosophie Positive," p. 619, M. Comte writes: "Le plus grand des métaphysiciens modernes, l'illustre Kant, a noblement mérité une éternelle admiration en tentant, le premier, d'échapper directement à l'absolu philosophique par sa célèbre conception de la double réalité, à la fois objective et subjective, qui indique un si juste sentiment de la saine philosophie."

But in the "Préface Personnelle" in the same volume, p. 35, M. Comte tells us: "Je n'ai jamais lu, en aucune langue, ni Vico, *ni Kant*, ni Herder, ni Hegel, etc.; je ne connais leurs divers ouvrages que d'après quelques relations indirectes et certains extraits fort insuffisants."

Who knows but that the "etc." may include Hume? And in that case, what is the value of M. Comte's praise of him?

language is to be found in the fact that, when I wrote, I had but just arisen from the perusal of a work with which he is doubtless well acquainted, M. Littré's "Auguste Comte et la Philosophie Positive."

Though there are tolerably fixed standards of right and wrong, and even of generosity and meanness, it may be said that the beauty or grandeur of a life is more or less a matter of taste; and Mr. Congreve's notions of literary excellence are so different from mine that it may be we should diverge as widely in our judgment of moral beauty or ugliness. Therefore, while retaining my own notions, I do not presume to quarrel with his. But when Mr. Congreve devotes a great deal of laboriously guarded insinuation to the endeavor to lead the public to believe that I have been guilty of the dishonesty of having criticised Comte without having read him, I must be permitted to remind him that he has neglected the well-known maxim of a diplomatic sage, "If you want to damage a man, you should say what is probable, as well as what is true."

And when Mr. Congreve speaks of my having an advantage over him in my introduction of "Christianity" into the phrase that "M. Comte's philosophy, in practice, might be described as Catholicism *minus* Christianity;" intending thereby to suggest that I have, by so doing, desired to profit by an appeal to the *odium theologicum*—he lays himself open to a very unpleasant retort.

What if I were to suggest that Mr. Congreve had not read Comte's works; and that the phrase "the context shows that the view of the writer ranges—however superficially—over the whole works. This is obvious from the mention of Catholicism," demonstrates that Mr. Congreve has no acquaintance with the "Philosophie Positive"? I think the suggestion would be very unjust and unmannerly, and I shall not make it. But the fact remains, that this little epigram of mine, which has so greatly provoked Mr. Congreve, is neither more nor less than a condensed paraphrase of the following passage which is to be found at page 344 of the fifth volume of the "Philosophie Positive:" *

"La seule solution possible de ce grand problème historique, qui n'a jamais pu être philosophiquement posé jusqu'ici, consiste à concevoir, en sens radicalement inverse des notions habituelles, *que ce qui devait nécessairement périr ainsi, dans le catholicisme, c'était la doctrine, et non l'organisation*, qui n'a été passagèrement ruinée que par suite de son inévitable adhérence élémentaire à la philosophie théologique, destinée à succomber graduellement sous l'irrésistible émancipation de la raison humaine; *tandis qu'une telle constitution, convenablement reconstruite sur des bases intellectuelles à la fois plus étendues et plus stables, devra finalement présider à l'indispensable réorganisation spirituelle des sociétés modernes, sauf les différences essentielles spontanément correspondantes à l'extrême diversité des doctrines fondamentales*; à moins de supposer, ce qui serait certainement contradictoire à l'ensemble des lois de notre nature, que les immenses efforts de tant de grands hommes, secondés par la persévérante sollicitude des nations civilisées, dans la fondation séculaire de ce chef-d'œuvre politique de la sagesse humaine, doivent être enfin irrévocablement perdus pour l'élite de l'humanité sauf les résultats, capitaux mais provisoires, qui s'y rapportaient immédiatement. Cette explication générale, déjà évidemment motivée par la suite des considérations propres à ce chapitre, sera de plus en plus confirmée par tout le reste de notre opération historique, *dont elle constituera spontanément la principale conclusion politique.*"

Nothing can be clearer. Comte's ideal, as stated by himself, is Catholic organization without Catholic doctrine, or, in other words, Catholicism *minus* Christianity. Surely it is utterly unjustifiable to ascribe to me base motives for stating a man's doctrines, as nearly as may be, in his own words!

My readers would hardly be interested were I to follow Mr. Congreve any further, or I might point out that the fact of his not having heard me lecture is hardly a safe ground for his speculations as to what I do not teach. Nor do I feel called upon to give any opinion as to M. Comte's merits or demerits as regards sociology. Mr. Mill (whose competence to speak on these matters I suppose will not be questioned, even by Mr. Congreve) has dealt with M. Comte's philosophy from this point of view, with a vigor and authority to which I cannot for a moment aspire; and with a severity, not unfrequently amounting to contempt, which I have not the wish, if I had the power to surpass. I, as a mere student in these ques-

* Now and always I quote the second edition, by Littré.

tron, am content to abide by Mr. Mill's judgment until some one shows cause for its reversal, and I decline to enter into a discussion which I have not provoked.

The sole obligation which lies upon me is to justify so much as still remains without justification of what I have written respecting Positivism—namely, the opinion expressed in the following paragraph :

"In so far as my study of what specially characterizes the Positive Philosophy has led me, I find therein little or nothing of any scientific value, and a great deal which is as thoroughly antagonistic to the very essence of science as anything in ultramontane Catholicism."

Here are two propositions : the first, that the "Philosophic Positive" contains little or nothing of any scientific value ; the second, that Comtism is, in spirit, anti-scientific. I shall endeavor to bring forward ample evidence in support of both.

I. No one who possesses even a superficial acquaintance with physical science can read Comte's "Leçons" without becoming aware that he was at once singularly devoid of real knowledge on these subjects, and singularly unlucky. What is to be thought of the contemporary of Young and of Fresnel, who never misses an opportunity of casting scorn upon the hypothesis of an ether—the fundamental basis not only of the undulatory theory of light, but of so much else in modern physics—and whose contempt for the intellects of some of the strongest men of his generation was such that he puts forward the mere existence of night as a refutation of the undulatory theory? What a wonderful gauge of his own value as a scientific critic does he afford by whom we are informed that phrenology is a great science, and psychology a chimera ; that Gall was one of the great men of his age, and that Cuvier was "brilliant but superficial !" * How unlucky must one consider the bold speculator who, just before the dawn of modern histology—which is simply the application of the microscope to anatomy—reproves what he calls "the abuse of microscopic investigations," and "the exaggerated credit" attached to them ; who, when the morphological uniformity of the tissues of the great majority of plants and animals was on the eve of being demonstrated, treated with ridicule those who attempt to refer all tissues to a "tissu générateur," formed by "le chimérique at inintelligible assemblage d'une sorte de monades organiques, qui seraient dès lors les vrais éléments primordiaux de tout corps vivant ;" and who finally tells us, that all the objections against a linear arrangement of the species of living beings are in their essence foolish, and that the order of the animal series is "necessarily linear," when the exact contrary is one of the best established and the most important truths of zoology. Appeal to mathematicians, astronomers, physicists, chemists, biologists, about the "Philosophic Positive," and they all, with one consent, begin to make protestation that, whatever M. Comte's other merits, he has shed no light upon the philosophy of their particular studies.

To be just, however, it must be admitted that even M. Comte's most ardent disciples are content to be judiciously silent about his knowledge or appreciation of the sciences themselves, and prefer to base their master's claims to scientific authority upon his "law of the three states," and his "classification of the sciences." But here, also, I must join issue with them as completely as others —notably Mr. Herbert Spencer—have done before me. A critical examination of what M. Comte has to say about the "law of the three states" brings out nothing but a series of more or less contradictory statements of an imperfectly apprehended truth ; and his "classification of the sciences," whether regarded historically or logically, is, in my judgment, absolutely worthless.

Let us consider the law of "the three states" as it is put before us in the opening of the first Leçon of the "Philosophie Positive :"

"En étudiant ainsi le développement total de l'intelligence humaine dans ses diverses sphères d'activité, depuis son premier essor le plus simple jusqu'à nos jours, je crois avoir découvert une grande loi fondamentale, à laquelle il est assujetti par une nécessité invariable, et qui me semble pouvoir être solidement établie, soit sur les preuves rationelles fournies par la connaissance de notre organisation, soit sur les vérifications historiques résultant d'un examen attentif du

passé. Cette loi consiste en ce que chacune de nos conceptions principales, chaque branche de nos connaissances, passe successivement par trois états théoriques différents; l'état théologique, ou fictif; l'état métaphysique, ou abstrait; l'état scientifique, ou positif. En d'autres termes, l'esprit humain, par sa nature, emploie successivement dans chacune de ses recherches trois methodes de philosopher, dont *le caractère est essentiellement différent et même radicalement* opposé; d'abord la méthode théologique, ensuite la méthode métaphysique, et enfin la méthode positive. De là, trois sortes de philosophie, ou de systèmes généraux de conceptions sur l'ensemble des phénomènes *qui s'excluent mutuellement*; la première est le point de départ nécessaire de l'intelligence humaine; la troisième, son état fixe et définitif; la seconde est uniquement destinée à servir de transition."

Nothing can be more precise than these statements, which may be put into the following propositions:

(*a*) The human intellect is subjected to the law by an invariable necessity which is demonstrable, *à priori*, from the nature and constitution of the intellect; while, as a matter of historical fact, the human intellect has been subjected to the law.

(*b*) Every branch of human knowledge passes through the three states, necessarily beginning with the first stage.

(*c*) The three states mutually exclude one another, being essentially different, and even radically opposed.

Two questions present themselves. Is M. Comte consistent with himself in making these assertions? And is he consistent with fact? I reply to both questions in the negative; and, as regards the first, I bring forward as my witness a remarkable passage which is to be found in the fourth volume of the "Philosophie Positive" (p. 491), when M. Comte had had time to think out, a little more fully, the notions crudely stated in the first volume:

"A proprement parler, la philosophie théologique, même dans notre première enfance, individuelle ou sociale, n'a jamais pu être rigoureusement universelle, c'est-à-dire que, pour les ordres quelconques de phénomènes, *les faits les plus simples et les plus communs ont toujours été regardés comme essentiellement assujettis à des lois naturelles, au lieu d'être attribués à l'arbitraire volonté des agents surnaturels.* L'illustre Adam Smith a, par exemple, très-heureusement remarqué dans ses essais philosophiques, qu'on ne trouvait en aucun temps ni en aucun pays, un dieu pour la pesanteur. *Il en est ainsi, en général, même à l'égard des sujets les plus compliqués, envers tous les phénomènes assez élémentaires et assez familiers pour que la parfaite invariabilité de leurs relations effectives ait toujours dû frapper spontanément l'observateur le moins préparé.* Dans l'ordre moral et social, qu'une vaine opposition voudrait aujourd'hui systématiquement interdire à la philosophie positive, il y a eu nécessairement, en tout temps, la pensée des lois naturelles, relativement aux plus simples phénomènes de la vie journalière, comme l'exige évidemment la conduite générale de notre existence réelle, individuelle ou sociale, qui n'aurait pu jamais comporter aucune prévoyance quelconque, si tous les phénomènes humains avaient été rigoureusement attribués à des agents surnaturels, puisque dès lors la prière aurait logiquement constitué la seule ressource imaginable pour influer sur le cours habituel des actions humaines. *On doit même remarquer, à ce sujet, que c'est au contraire, l'ebauche spontanée des premières lois naturelles propres aux actes individuels ou sociaux qui, fictivement transportée à tous les phénomènes du monde extérieur, a d'abord fourni, d'après nos explications précédentes, le vrai principe fondamental de la philosophie théologique. Ainsi, le germe élémentaire de la philosophie positive est certainement tout aussi primitif au fond que celui de la philosophie théologique elle-même, quoi qu'il n'ait pu se développer que beaucoup plus tard.* Une telle notion importe extrêmement à la parfaite rationalité de notre théorie sociologique, puisque la vie humaine ne pouvant jamais offrir aucune véritable création quelconque, mais toujours une simple évolution graduelle, l'essor final de l'esprit positif deviendrait scientifiquement incompréhensible, si, dès l'origine, on n'en concevait, à tous égards, les premiers rudiments nécessaires. Depuis cette situation primitive, à mesure que nos observations se sont spontanément étendues et généralisées, cet essor, d'abord à peine appréciable, a constamment suivi, sans cesser longtemps d'être subalterne, une progression très-lente, mais continue, la philosophie théologique restant toujours réservée pour les phénomènes, de moins en moins nombreux, dont les lois naturelles ne pouvaient encore être aucunement connues."

Compare the propositions implicitly laid down here with those contained in the earlier volume. (*a*) As a matter of fact, the human intellect has *not* been invariably subjected to the law of the three states, and therefore the necessity of the law *cannot* be demonstrable *à priori*. (*b*) Much of our knowledge of all kinds has *not* passed through the three states, and more particularly, as M. Comte is careful to point out, not through the first. (*c*) The positive state has more or less co-existed with the theological, from the dawn of human intelligence. And, by

way of completing the series of contradictions, the assertion that the three states are "essentially different and even radically opposed," is met a little lower on the same page by the declaration that "the metaphysical state is, at bottom, nothing but a simple general modification of the first;" while, in the fortieth Leçon, as also in the interesting early essay entitled "Considérations philosophiques sur les Sciences et les Savants (1825)," the three states are practically reduced to two. "Le véritable esprit général de toute philosophie théologique ou métaphysique consiste à prendre pour principe, dans l'explication des phénomènes du monde extérieur, notre sentiment immédiat des phénomènes humaines; tandis que au contraire, la philosophie positive est toujours caractérisée, non moins profondément, par la subordination nécessaire et rationelle de la conception de l'homme à celle du monde."

I leave M. Comte's disciples to settle which of these contradictory statements expresses their master's real meaning. All I beg leave to remark is, that men of science are not in the habit of paying much attention to "laws" stated in this fashion.

The second statement is undoubtedly far more rational and consistent with fact than the first; but I cannot think it is a just or adequate account of the growth of intelligence, either in the individual man or in the human species. Any one who will carefully watch the development of the intellect of a child will perceive that, from the first, its mind is mirroring nature in two different ways. On the one hand, it is merely drinking in sensations and building up associations, while it forms conceptions of things and their relations which are more thoroughly "positive," or devoid of entanglement with hypotheses of any kind, than they will ever be in after life. No child has recourse to imaginary personifications in order to account for the ordinary properties of objects which are not alive, or do not represent living things. It does not imagine that the taste of sugar is brought about by a god of sweetness, or that a spirit of jumping causes a ball to bound. Such phenomena, which form the basis of a very large part of its ideas, are taken as matters of course—as ultimate facts which suggest no difficulty and need no explanation. So far as all these common though important phenomena are concerned, the child's mind is in what M. Comte would call the "positive" state.

But, side by side with this mental condition, there rises another. The child becomes aware of itself as a source of action and a subject of passion and of thought. The acts which follow upon its own desires are among the most interesting and prominent of surrounding occurrences; and these acts, again, plainly arise either out of affections caused by surrounding things or of other changes in itself. Among these surrounding things, the most interesting and important are mother and father, brethren and nurses. The hypothesis that these wonderful creatures are of like nature to itself is speedily forced upon the child's mind; and this primitive piece of anthropomorphism turns out to be a highly successful speculation, which finds its justification at every turn. No wonder, then, that it is extended to other similarly interesting objects which are not too unlike these—to the dog, the cat, and the canary, the doll, the toy, and the picture-book—that these are endowed with wills and affections, and with capacities for being "good" and "naughty." But surely it would be a mere perversion of language to call this a "theological" state of mind, either in the proper sense of the word "theological," or as contrasted with "scientific" or "positive." The child does not worship either father or mother, dog or doll. On the contrary, nothing is more curious than the absolute irreverence, if I may so say, of a kindly-treated young child; its tendency to believe in itself as the centre of the universe, and its disposition to exercise despotic tyranny over those who could crush it with a finger.

Still less is there anything unscientific, or anti-scientific, in this infantile anthropomorphism. The child observes that many phenomena are the consequences of affections of itself; it soon has excellent reasons for the belief that many other phenomena are consequences of the affections of other beings, more or less like itself. And having thus good evidence for believing that many of the most interesting occurrences about it are explica-

ble on the hypothesis that they are the work of intelligences like itself—having discovered a *vera causa* for many phenomena—why should the child limit the application of so fruitful an hypothesis? The dog has a sort of intelligence, so has the cat; why should not the doll and the picture-book also have a share, proportioned to their likeness to intelligent things?

The only limit which does arise is exactly that which, as a matter of science, should arise; that is to say, the anthropomorphic interpretation is applied only to those phenomena which, in their general nature or their apparent capriciousness, resemble those which the child observes to be caused by itself, or by beings like itself. All the rest are regarded as things which explain themselves, or are inexplicable.

It is only at a later stage of intellectual development that the intelligence of man awakes to the apparent conflict between the anthropomorphic, and what I may call the physical,* aspect of nature, and either endeavors to extend the anthropomorphic view over the whole of nature—which is the tendency of theology; or to give the same exclusive predominance to the physical view—which is the tendency of science; or adopts a middle course, and taking from the anthropomorphic view its tendency to personify, and from the physical view its tendency to exclude volition and affection, ends in what M. Comte calls the "metaphysical" state—"metaphysical," in M. Comte's writings, being a general term of abuse for anything he does not like.

What is true of the individual is, *mutatis mutandis*, true of the intellectual development of the species. It is absurd to say of men in a state of primitive savagery, that all their conceptions are in a theological state. Nine-tenths of them are eminently realistic, and as "positive" as ignorance and narrowness can make them. It no more occurs to a savage than it does to a child, to ask the why of the daily and ordinary occurrences which form the greater part of his mental life. But in regard to the more striking or out-of-the-way events, which force him to speculate, he is highly anthropomorphic; and, as compared with a child, his anthropomorphism is complicated by the intense impression which the death of his own kind makes upon him, as indeed it well may. The warrior, full of ferocious energy, perhaps the despotic chief of his tribe, is suddenly struck down. A child may insult the man a moment before so awful; a fly rests, undisturbed, on the lips from which undisputed command issued. And yet the bodily aspect of the man seems hardly more altered than when he slept, and, sleeping, seemed to himself to leave his body and wander through dreamland. What then if that something, which is the essence of the man, has really been made to wander by the violence done to it, and is unable, or has forgotten, to come back to its shell? Will it not retain somewhat of the powers it possessed during life? May it not help us if it be pleased, or (as seems to be by far the more general impression) hurt us if it be angered? Will it not be well to do toward it those things which would have soothed the man and put him in good humor during his life? It is impossible to study trustworthy accounts of savage thought without seeing that some such train of ideas as this lies at the bottom of their speculative beliefs.

There are savages without God, in any proper sense of the word, but none without ghosts. And the Fetishism, Ancestor-worship, Hero-worship, and Demonology of primitive savages, are all, I believe, different manners of expression of their belief in ghosts, and of the anthropomorphic interpretation of out-of-the-way events, which is its concomitant. Witchcraft and sorcery are the practical expressions of these beliefs; and they stand in the same relation to religious worship as the simple anthropomorphism of children, or savages, does to theology.

In the progress of the species from

* The word "positive" is in every way objectionable. In one sense it suggests that mental quality which was undoubtedly largely developed in M. Comte, but can best be dispensed with in a philosopher: in another, it is unfortunate in its application to a system which starts with enormous negations; in its third, and specially philosophical sense, as implying a system of thought which assumes nothing beyond the content of observed facts, it implies that which never did exist, and never will.

savagery to advanced civilization, anthropomorphism grows into theology, while physicism (if I may so call it) develops into science; but the development of the two is contemporaneous, not successive. For each, there long exists an assured province which is not invaded by the other; while, between the two, lies a debatable land, ruled by a sort of bastards, who owe their complexion to physicism and their substance to anthropomorphism, and are M. Comte's particular aversions—metaphysical entities.

But, as the ages lengthen, the borders of Physicism increase. The territories of the bastards are all annexed to science; and even Theology, in her purer forms, has ceased to be anthropomorphic, however she may talk. Anthropomorphism has taken stand in its last fortress—man himself. But science closely invests the walls; and Philosophers gird themselves for battle upon the last and greatest of all speculative problems—Does human nature possess any free, volitional, or truly anthropomorphic element, or is it only the cunningest of all Nature's clocks? Some, among whom I count myself, think that the battle will forever remain a drawn one, and that, for all practical purposes, this result is as good as anthropomorphism winning the day.

The classification of the sciences, which, in the eyes of M. Comte's adherents, constitutes his second great claim to the dignity of a scientific philosopher, appears to me to be open to just the same objections as the law of the three states. It is inconsistent in itself, and it is inconsistent with fact. Let us consider the main points of this classification successively:

"Il faut distinguer par rapport à tous les ordres des phénomènes, deux genres de sciences naturelles; les unes abstraites, générales, ont pour objet la découverte des lois qui régissent les diverses classes de phénomènes, en considérant tous les cas qu'on peut concevoir; les autres concrètes, particulières, descriptives, et qu'on désigne quelquefois sous le nom des sciences naturelles proprement dites, consistent dans l'application de ces lois à l'histoire effective des différents êtres existants."

The "abstract" sciences are subsequently said to be mathematics, astronomy, physics, chemistry, physiology, and social physics—the titles of the two latter being subsequently changed to biology and sociology. M. Comte exemplifies the distinction between his abstract and his concrete sciences as follows:

"On pourra d'abord l'apercevoir très-nettement en comparant, d'une part, la physiologie générale, et d'une autre part la zoologie et la botanique proprement dites. Ce sont évidemment, en effet, deux travaux d'un caractère fort distinct, que d'étudier, en général, les lois de la vie, ou de déterminer le mode d'existence de chaque corps vivant, en particulier. *Cette seconde étude, en outre, est nécessairement fondée sur la première.*"—P. 57.

All the unreality and mere bookishness of M. Comte's knowledge of physical science comes out in the passage I have italicized. "The special study of living beings is based upon a general study of the laws of life!" What little I know about the matter leads me to think that, if M. Comte had possessed the slightest practical acquaintance with biological science, he would have turned his phraseology upside down, and have perceived that we can have no knowledge of the general laws of life, except that which is based upon the study of particular living beings.

The illustration is surely unluckily chosen; but the language in which these so-called abstract sciences are defined seems to me to be still more open to criticism. With what propriety can astronomy, or physics, or chemistry, or biology, be said to occupy themselves with the consideration of "all conceivable cases" which fall within their respective provinces? Does the astronomer occupy himself with any other system of the universe than that which is visible to him? Does he speculate upon the possible movements of bodies which may attract one another in the inverse proportion of the cube of their distances, say? Does biology, whether "abstract" or "concrete," occupy itself with any other form of life than those which exist, or have existed? And, if the abstract sciences embrace all conceivable cases of the operation of the laws with which they are concerned, would not they, necessarily, embrace the subjects of the concrete sciences, which, inasmuch as they exist, must needs be conceivable? In fact, no such distinction as that which M. Comte draws is tenable. The first

stage of his classification breaks by its own weight.

But granting M. Comte his six abstract sciences, he proceeds to arrange them according to what he calls their natural order or hierarchy, their places in this hierarchy being determined by the degree of generality and simplicity of the conceptions with which they deal. Mathematics occupies the first, astronomy the second, physics the third, chemistry the fourth, biology the fifth, and sociology the sixth and last place in the series. M. Comte's arguments in favor of this classification are first—

"Sa conformité essentielle avec la co-ordination, en quelque sorte spontanée, qui se trouve en effet implicitement admise par les savants livrès à l'étude des diverse branches de la philosophie naturelle."

But I absolutely deny the existence of this conformity. If there is one thing clear about the progress of modern science, it is the tendency to reduce all scientific problems, except those which are purely mathematical, to questions of molecular physics—that is to say, to the attractions, repulsions, motions, and co-ordination of the ultimate particles of matter. Social phenomena are the result of the interaction of the components of society, or men, with one another and the surrounding universe. But, in the language of physical science, which, by the nature of the case is materialistic, the actions of men, so far as they are recognizable by science, are the results of molecular changes in the matter of which they are composed; and, in the long run, these must come into the hands of the physical. *A fortiori*, the phenomena of biology and of chemistry are, in their ultimate analysis, questions of molecular physics. Indeed, the fact is acknowledged by all chemists and biologists who look beyond their immediate occupations. And it is to be observed, that the phenomena of biology are as directly and immediately connected with molecular physics as are those of chemistry. Molar physics, chemistry, and biology are not three successive steps in the ladder of knowledge, as M. Comte would have us believe, but three branches springing from the common stem of molecular physics. As to astronomy, I am at a loss to understand how any one who will give a moment's attention to the nature of the science can fail to see that it consists of two parts: first, of a description of the phenomena, which is as much entitled as descriptive zoology, or botany, is, to the name of natural history; and secondly, of an explanation of the phenomena, furnished by the laws of a force—gravitation—the study of which is as much a part of physics as is that of heat, or electricity. It would be just as reasonable to make the study of the heat of the sun a science preliminary to the rest of thermotics, as to place the study of the attraction of the bodies which compose the universe in general before that of the particular terrestrial bodies which alone we can experimentally know. Astronomy, in fact, owes its perfection to the circumstance that it is the only branch of natural history, the phenomena of which are largely expressible by mathematical conceptions, and which can be, to a great extent, explained by the application of very simple physical laws.

With regard to mathematics, it is to be observed, in the first place, that M. Comte mixes up under that head the pure relations of space and of quantity, which are properly included under the name, with rational mechanics and statics, which are mathematical developments of the most general conceptions of physics, namely, the notions of force and of motion. Relegating these to their proper place in physics, we have left pure mathematics, which can stand neither at the head, nor at the tail, of any hierarchy of the sciences, since, like logic, it is equally related to all; though the enormous practical difficulty of applying mathematics to the more complex phenomena of nature removes them, for the present, out of its sphere.

On this subject of mathematics, again, M. Comte indulges in assertions which can only be accounted for by his total ignorance of physical science practically. As for example:

"C'est donc par l'étude des mathématiques, *et seulement par elle*, que l'on peut se faire une idée juste et approfondie de ce que c'est qu'une *science*. C'est là *uniquement* qu'on doit chercher à connaître avec précision *la méthode générale que l'esprit humain emploie constamment dans toutes ses recherches positives*, parce que nulle part ailleurs les questions ne sont résolues d'une manière

aussi complèté et les déductions prolongées aussi loin avec une sévérité rigoureuse. C'est là également que notre entendement a donné les plus grandes preuves de sa force, parce que les idées qu'il y considère sont du plus haut degré d'abstraction possible dans l'ordre positif. *Toute éducation scientifique qui ne commence point par une telle étude pèche donc nécessairement par sa base.*"

That is to say, the only study which can confer "a just and comprehensive idea of what is meant by science," and, at the same time, furnish an exact conception of the general method of scientific investigation, is that which knows nothing of observation, nothing of experiment, nothing of induction, nothing of causation! And education, the whole secret of which consists in proceeding from the easy to the difficult, the concrete to the abstract, ought to be turned the other way, and pass from the abstract to the concrete.

M. Comte puts a second argument in favor of his hierarchy of the sciences thus:

"Un second caractère très-essentiel de notre classification, c'est d'être nécessairement conforme à l'ordre effectif du développement de la philosophie naturelle. C'est ce que vérifie tout ce qu'on sait de l'histoire des sciences."

But Mr. Spencer has so thoroughly and completely demonstrated the absence of any correspondence between the historical development of the sciences, and their position in the Comtean hierarchy, in his essay on the "Genesis of Science," that I shall not waste time in repeating his refutation.

A third proposition in support of the Comtean classification of the sciences stands as follows:

"En troisième lieu cette classification présente la propriété très-remarquable de marquer exactement la perfection relative des différentes sciences, laquelle consiste essentiellement dans le degré de précision des connaissances et dans leur co-ordination plus ou moins intime."

I am quite unable to understand the distinction which M. Comte endeavors to draw in this passage, in spite of his amplifications further on. Every science must consist of precise knowledge, and that knowledge must be co-ordinated into general proportions, or it is not science. When M. Comte, in exemplification of the statement I have cited, says that "les phénomènes organiques ne comportent qu'une étude à la fois moins exacte et moins systématique que les phénomènes des corps bruts," I am at a loss to comprehend what he means. If I affirm that "when a motor nerve is irritated, the muscle connected with it becomes simultaneously shorter and thicker, without changing its volume," it appears to me that the statement is as precise or exact (and not merely as true) as that of the physicist who should say, that "when a piece of iron is heated, it becomes simultaneously longer and thicker and increases in volume;" nor can I discover any difference, in point of precision, between the statement of the morphological law that "animals which suckle their young have two occipital condyles," and the enunciation of the physical law that "water subjected to electrolysis is replaced by an equal weight of the gases, oxygen and hydrogen." As for anatomical or physiological investigation being less "systematic" than that of the physicist or chemist, the assertion is simply unaccountable. The methods of physical science are everywhere the same in principle, and the physiological investigator who was not "systematic" would, on the whole, break down rather sooner than the inquirer into simpler subjects.

Thus M. Comte's classification of the sciences, under all its aspects, appears to me to be a complete failure. It is impossible, in an article which is already too long, to inquire how it may be replaced by a better; and it is the less necessary to do so, as a second edition of Mr. Spencer's remarkable essay on this subject has just been published. After wading through pages of the long-winded confusion and second-hand information of the "Philosophie Positive," at the risk of a *crise cérébrale*—it is as good as a shower-bath to turn to the "Classification of the Sciences," and refresh oneself with Mr. Spencer's profound thought, precise knowledge, and clear language.

II. The second proposition to which I have committed myself, in the paper to which I have been obliged to refer so often, is, that the "Positive Philosophy" contains "a great deal which is as thoroughly antagonistic to the very essence of science as is anything in ultramontane Catholicism."

What I refer to in these words, is, on the one hand, the dogmatism and narrowness which so often mark M. Comte's discussion of doctrines which he does not like, and reduce his expressions of opinion to mere passionate puerilities; as, for example, when he is arguing against the assumption of an ether, or when he is talking (I cannot call it arguing) against psychology, or political economy. On the other hand, I allude to the spirit of meddling systematization and regulation which animates even the "Philosophie Positive," and breaks out, in the latter volumes of that work, into no uncertain foreshadowing of the anti scientific monstrosities of Comte's later writings.

Those who try to draw a line of demarcation between the spirit of the "Philosophie Positive," and that of the "Politique" and its successors (if I may express an opinion from fragmentary knowledge of these last), must have overlooked or forgotten what Comte himself labors to show, and indeed succeeds in proving, in the "Appendice Général" of the "Politique Positive." "Dès mon début," he writes, "je tentai de fonder le nouveau pouvoir spirituel que j'institue aujourd'hui." "Ma politique, loin d'être aucunement opposée à ma philosophie, en constitue tellement la suite naturelle que celle-ci fut directement instituée pour servir de base à celle-là, comme le prouve cet appendice."

This is quite true. In the remarkable essay entitled "Considérations sur le Pouvoir spirituel," published in March, 1826, Comte advocates the establishment of a "modern spiritual power," which, he anticipates, may exercise an even greater influence over temporal affairs than did the Catholic clergy, at the height of their vigor and independence, in the twelfth century. This spiritual power is, in fact, to govern opinion, and to have the supreme control over education, in each nation of the West; and the spiritual powers of the several European peoples are to be associated together and placed under a common direction or "souveraineté spirituelle."

A system of "Catholicism *minus* Christianity" was therefore completely organized in Comte's mind, four years before the first volume of the "Philosophie Positive" was written; and naturally, the papal spirit shows itself in that work, not only in the ways I have already mentioned, but, notably, in the attack on liberty of conscience which breaks out in the fourth volume:

"Il n'y a point de liberté de conscience en astronomie, en physique, en chimie, en physiologie même, en ce sens que chacun trouverait absurde de ne pas croire de confiance aux principes établis dans les sciences par les hommes compétents."

"Nothing in ultramontane Catholicism" can, in my judgment, be more completely sacerdotal, more entirely anti-scientific, than this dictum. All the great steps in the advancement of science have been made by just those men who have not hesitated to doubt the "principles established in the sciences by competent persons;" and the great teaching of science—the great use of it as an instrument of mental discipline—is its constant inculcation of the maxim, that the sole ground on which any statement has a right to be believed is the impossibility of refuting it.

Thus, without travelling beyond the limits of the "Philosophie Positive," we find its author contemplating the establishment of a system of society, in which an organized spiritual power shall override and direct the temporal power, as completely as the Innocents and Gregorys tried to govern Europe in the middle ages; and repudiating the exercise of liberty of conscience against the "*hommes compétents,*" of whom, by the assumption, the new priesthood would be composed. Was Mr. Congreve as forgetful of this, as he seems to have been of some other parts of the "Philosophie Positive," when he wrote, that "in any limited, careful use of the term, no candid man could say that the Positive Philosophy contained a great deal as thoroughly antagonistic to [the very essence of *] science as Catholicism"?

M. Comte, it will have been observed, desires to retain the whole of Catholic organization; and the logical practical result of this part of his doctrine would be the establishment of something corresponding with that eminently Catholic, but admittedly anti-scientific, institution

* Mr. Congreve leaves out these important words, which show that I refer to the spirit, and not to the details of science.

—the Holy Office.

I hope I have said enough to show that I wrote the few lines I devoted to M. Comte and his philosophy, neither unguardedly nor ignorantly, still less maliciously. I shall be sorry if what I have now added, in my own justification, should lead any to suppose that I think M. Comte's works worthless; or that I do not heartily respect and sympathize with those who have been impelled by him to think deeply upon social problems, and to strive nobly for social regeneration. It is the virtue of that impulse, I believe, which will save the name and fame of Auguste Comte from oblivion. As for his philosophy, I part with it by quoting his own words, reported to me by a quondam Comtist, now an eminent member of the Institute of France, M. Charles Robin:

"La Philosophie est une tentative incessante de l'esprit humain pour arriver au repos: mais elle se trouve incessamment aussi dérangée par les progrès continus de la science. De là vient pour le philosophe l'obligation de refaire chaque soir la synthèse de ses conceptions; et un jour viendra où l'homme raisonnable ne fera plus d'autre prière du soir."

ON A PIECE OF CHALK.

A LECTURE TO WORKING MEN.

If a well were to be sunk at our feet in the midst of the city of Norwich, the diggers would very soon find themselves at work in that white substance almost too soft to be called rock, with which we are all familiar as "chalk."

Not only here, but over the whole county of Norfolk, the well-sinker might carry his shaft down many hundred feet without coming to the end of the chalk; and, on the sea-coast, where the waves have pared away the face of the land which breasts them, the scarped faces of the high cliffs are often wholly formed of the same material. Northward, the chalk may be followed as far as Yorkshire; on the south coast it appears abruptly in the picturesque western bays of Dorset, and breaks into the Needles of the Isle of Wight; while on the shores of Kent it supplies that long line of white cliffs to which England owes her name of Albion.

Were the thin soil which covers it all washed away, a curved band of white chalk, here broader, and there narrower, might be followed diagonally across England from Lulworth in Dorset, to Flamborough Head in Yorkshire—a distance of over 280 miles as the crow flies.

From this band to the North Sea, on the east, and the Channel, on the south, the chalk is largely hidden by other deposits; but, except in the Weald of Kent and Sussex, it enters into the very foundation of all the south-eastern counties.

Attaining, as it does in some places, a thickness of more than a thousand feet, the English chalk must be admitted to be a mass of considerable magnitude. Nevertheless, it covers but an insignificant portion of the whole area occupied by the chalk formation of the globe, which has precisely the same general characters as ours, and is found in detached patches, some less, and others more extensive, than the English.

Chalk occurs in north-west Ireland; it stretches over a large part of France—the chalk which underlies Paris being, in fact, a continuation of that of the London basin; it runs through Denmark and Central Europe, and extends southward to North Africa; while eastward, it appears in the Crimea and in Syria, and may be traced as far as the shores of the Sea of Aral, in Central Asia.

If all the points at which true chalk occurs were circumscribed, they would lie within an iregular oval about 3000 miles in long diameter—the area of which would be as great as that of Europe, and would many times exceed that of the largest existing inland sea—the Mediterranean.

Thus the chalk is no unimportant element in the masonry of the earth's crust, and it impresses a peculiar stamp, varying with the conditions to which it is exposed, on the scenery of the districts in which it occurs. The undulating downs and rounded coombs, covered with sweet-grassed turf, of our inland chalk country, have a peacefully domestic and mutton-suggesting prettiness, but can hardly be called either grand or beautiful. But on our southern coasts, the wall-sided cliffs, many hundred feet high, with vast needles and pinnacles

standing out in the sea, sharp and solitary enough to serve as perches for the wary cormorant, confer a wonderful beauty and grandeur upon the chalk headlands. And in the East, chalk has its share in the formation of some of the most venerable of mountain ranges, such as the Lebanon.

What is this wide-spread component of the surface of the earth? and whence did it come?

You may think this no very hopeful inquiry. You may not unnaturally suppose that the attempt to solve such problems as these can lead to no result, save that of entangling the inquirer in vague speculations, incapable of refutation and of verification.

If such were really the case, I should have selected some other subject than a "piece of chalk" for my discourse. But, in truth, after much deliberation, I have been unable to think of any topic which would so well enable me to lead you to see how solid is the foundation upon which some of the most startling conclusions of physical science rest.

A great chapter of the history of the world is written in the chalk. Few passages in the history of man can be supported by such an overwhelming mass of direct and indirect evidence as that which testifies to the truth of the fragment of the history of the globe, which I hope to enable you to read, with your own eyes, to-night.

Let me add, that few chapters of human history have a more profound significance for ourselves. I weigh my words well when I assert, that the man who should know the true history of the bit of chalk which every carpenter carries about in his breeches-pocket, though ignorant of all other history, is likely, if he will think his knowledge out to its ultimate results, to have a truer, and therefore a better, conception of this wonderful universe, and of man's relation to it, than the most learned student who is deep-read in the records of humanity and ignorant of those of nature.

The language of the chalk is not hard to learn, not nearly so hard as Latin, if you only want to get at the broad features of the story it has to tell; and I propose that we now set to work to spell that story out together.

We all know that if we "burn" chalk, the result is quicklime. Chalk, in fact, is a compound of carbonic acid gas and lime; and when you make it very hot, the carbonic acid flies away and the lime is left.

By this method of procedure we see the lime, but we do not see the carbonic acid. If, on the other hand, you were to powder a little chalk and drop it into a good deal of strong vinegar, there would be a great bubbling and fizzing, and, finally, a clear liquid, in which no sign of chalk would appear. Here you see the carbonic acid in the bubbles; the lime, dissolved in the vinegar, vanishes from sight. There are a great many other ways of showing that chalk is essentially nothing but carbonic acid and quicklime. Chemists enunciate the result of all the experiments which prove this, by stating that chalk is almost wholly composed of "carbonate of lime."

It is desirable for us to start from the knowledge of this fact, though it may not seem to help us very far toward what we seek. For carbonate of lime is a widely-spread substance, and is met with under very various conditions. All sorts of limestones are composed of more or less pure carbonate of lime. The crust which is often deposited by waters which have drained through limestone rocks, in the form of what are called stalagmites and stalactites, is carbonate of lime. Or, to take a more familiar example, the fur on the inside of a tea-kettle is carbonate of lime; and, for anything chemistry tells us to the contrary, the chalk might be a kind of gigantic fur upon the bottom of the earth-kettle, which is kept pretty hot below.

Let us try another method of making the chalk tell us its own history. To the unassisted eye chalk looks simply like a very loose and open kind of stone. But it is possible to grind a slice of chalk down so thin that you can see through it—until it is thin enough, in fact, to be examined with any magnifying power that may be thought desirable. A thin slice of the fur of a kettle might be made in the same way. If it were examined microscopically, it would show itself to be a more or less distinctly laminated mineral substance, and nothing more.

But the slice of chalk presents a totally different appearance when placed under the microscope. The general mass of it is made up of very minute granules; but, imbedded in this matrix, are innumerable bodies, some smaller and some larger, but, on a rough average, not more than a hundredth of an inch in diameter, having a well-defined shape and structure. A cubic inch of some specimens of chalk may contain hundreds of thousands of these bodies, compacted together with incalculable millions of the granules.

The examination of a transparent slice gives a good notion of the manner in which the components of the chalk are arranged, and of their relative proportions. But, by rubbing up some chalk with a brush in water and then pouring off the milky fluid, so as to obtain sediments of different degrees of fineness, the granules and the minute rounded bodies may be pretty well separated from one another, and submitted to microscopic examination, either as opaque or as transparent objects. By combining the views obtained in these various methods, each of the rounded bodies may be proved to be a beautifully-constructed calcareous fabric, made up of a number of chambers, communicating freely with one another. The chambered bodies are of various forms. One of the commonest is something like a badly-grown raspberry, being formed of a number of nearly globular chambers of different sizes congregated together. It is called *Globigerina*, and some specimens of chalk consist of little else than *Globigerinæ* and granules.

Let us fix our attention upon the *Globigerina*. It is the spoor of the game we are tracking. If we can learn what it is and what are the conditions of its existence, we shall see our way to the origin and past history of the chalk.

A suggestion which may naturally enough present itself is, that these curious bodies are the result of some process of aggregation which has taken place in the carbonate of lime; that, just as in winter, the rime on our windows simulates the most delicate and elegantly arborescent foliage — proving that the mere mineral matter may, under certain conditions, assume the outward form of organic bodies — so this mineral substance, carbonate of lime, hidden away in the bowels of the earth, has taken the shape of these chambered bodies. I am not raising a merely fanciful and unreal objection. Very learned men, in former days, have even entertained the notion that all the formed things found in rocks are of this nature; and if no such conception is at present held to be admissible, it is because long and varied experience has now shown that mineral matter never does assume the form and structure we find in fossils. If anyone were to try to persuade you that an oyster-shell (which is also chiefly composed of carbonate of lime) had crystallized out of sea-water, I suppose you would laugh at the absurdity. Your laughter would be justified by the fact that all experience tends to show that oyster-shells are formed by the agency of oysters, and in no other way. And if there were no better reasons, we should be justified, on like grounds, in believing that *Globigerina* is not the product of anything but vital activity.

Happily, however, better evidence in proof of the organic nature of the *Globigerinæ* than that of analogy is forthcoming. It so happens that calcareous skeletons, exactly similar to the *Globigerinæ* of the chalk, are being formed, at the present moment, by minute living creatures, which flourish in multitudes, literally more numerous than the sands of the sea-shore, over a large extent of that part of the earth's surface which is covered by the ocean.

The history of the discovery of these living *Globigerinæ*, and of the part which they play in rock building, is singular enough. It is a discovery which, like others of no less scientific importance, has arisen, incidentally, out of work devoted to very different and exceedingly practical interests.

When men first took to the sea, they speedily learned to look out for shoals and rocks; and the more the burthen of their ships increased, the more imperatively necessary it became for sailors to ascertain with precision the depth of the waters they traversed. Out of this necessity grew the use of the lead and sounding line; and, ultimately, marine-surveying, which is the recording of the form of coasts and of the depth of the sea, as ascertained by the sounding-lead, upon

charts.

At the same time, it became desirable to ascertain and to indicate the nature of the sea-bottom, since this circumstance greatly affects its goodness as holding ground for anchors. Some ingenious tar, whose name deserves a better fate than the oblivion into which it has fallen attained this object by "arming" the bottom of the lead with a lump of grease, to which more or less of the sand or mud, or broken shells, as the case might be, adhered, and was brought to the surface. But, however well adapted such an apparatus might be for rough nautical purposes, scientific accuracy could not be expected from the armed lead, and to remedy its defects (especially when applied to sounding in great depths) Lieut. Brooke, of the American Navy, some years ago invented a most ingenious machine, by which a considerable portion of the superficial layer of the sea-bottom can be scooped out and brought up, from any depth to which the lead descends.

In 1853, Lieut. Brooke obtained mud from the bottom of the North Atlantic, between Newfoundland and the Azores, at a depth of more than 10,000 feet or two miles, by the help of this sounding apparatus. The specimens were sent for examination to Ehrenberg of Berlin, and to Bailey of West Point, and those able microscopists found that this deep-sea mud was almost entirely composed of the skeletons of living organisms—the greater proportion of these being just like the *Globigerinæ* already known to occur in the chalk.

Thus far, the work had been carried on simply in the interests of science, but Lieut. Brooke's method of sounding acquired a high commercial value, when the enterprise of laying down the telegraph-cable between this country and the United States was undertaken. For it became a matter of immense importance to know, not only the depth of the sea over the whole line along which the cable was to be laid, but the exact nature of the bottom, so as to guard against chances of cutting or fraying the strands of that costly rope. The Admiralty consequently ordered Captain Dayman, an old friend and shipmate of mine, to ascertain the depth over the whole line of the cable, and to bring back specimens of the bottom. In former days, such a command as this might have sounded very much like one of the impossible things which the young prince in the Fairy Tales is ordered to do before he can obtain the hand of the princess. However, in the months of June and July, 1857, my friend performed the task assigned to him with great expedition and precision, without, so far as I know, having met with any reward of that kind. The specimens of Atlantic mud which he procured were sent to me to be examined and reported upon.*

The result of all these operations is, that we know the contours and the nature of the surface-soil covered by the North Atlantic, for a distance of 1700 miles from east to west, as well as we know that of any part of the dry land.

It is a prodigious plain—one of the widest and most even plains in the world. If the sea were drained off, you might drive a wagon all the way from Valentia, on the west coast of Ireland, to Trinity Bay, in Newfoundland. And, except upon one sharp incline about 200 miles from Valentia, I am not quite sure that it would even be necessary to put the skid on, so gentle are the ascents and descents upon that long route. From Valentia the road would lie down-hill for about 200 miles to the point at which the bottom is now covered by 1700 fathoms of sea-water. Then would come the central plain, more than a thousand miles wide, the inequalities of the surface of which would be hardly perceptible, though the depth of water upon it now varies from 10,000 to 15,000 feet; and there are places in which Mont Blanc might be sunk without showing its peak above water. Beyond this, the ascent on the American side commences, and gradually leads, for about 300 miles, to the Newfoundland shore.

Almost the whole of the bottom of this central plain (which extends for many hundred miles in a north and south

* See Appendix to Captain Dayman's "Deep-sea Soundings in the North Atlantic Ocean, between Ireland and Newfoundland, made in H.M.S. Cyclops. Published by order of the Lords Commissioners of the Admiralty, 1858." They have since formed the subject of an elaborate memoir by Messrs. Parker and Jones, published in the *Philosophical Transactions* for 1865.

direction) is covered by a fine mud, which when brought to the surface, dries into a grayish, white friable substance. You can write with this on a blackboard, if you are so inclined; and, to the eye, it is quite like very soft, grayish chalk. Examined chemically, it proves to be composed almost wholly of carbonate of lime; and if you make a section of it, in the same way as that of the piece of chalk was made, and view it with the microscope, it presents innumerable *Globigerinæ* embedded in a granular matrix.

Thus this deep-sea mud is substantially chalk. I say substantially, because there are a good many minor differences; but as these have no bearing on the question immediately before us—which is the nature of the *Globigerinæ* of the chalk—it is unnecessary to speak of them.

Globigerinæ of every size, from the smallest to the largest, are associated together in the Atlantic mud, and the chambers of many are filled by a soft animal matter. This soft substance is, in fact, the remains of the creature to which the *Globigerina* shell, or rather skeleton, owes its existence—and which is an animal of the simplest imaginable description. It is, in fact, a mere particle of living jelly, without defined parts of any kind—without a mouth, nerves, muscles, or distinct organs, and only manifesting its vitality to ordinary observation by thrusting out and retracting from all parts of its surface long filamentous processes, which serve for arms and legs. Yet this amorphous particle, devoid of everything which, in the higher animals, we call organs, is capable of feeding, growing, and multiplying; of separating from the ocean the small proportion of carbonate of lime which is dissolved in sea-water; and of building up that substance into a skeleton for itself, according to a pattern which can be imitated by no other known agency.

The notion that animals can live and flourish in the sea, at the vast depths from which apparently living *Globigerinæ* have been brought up, does not agree very well with our usual conceptions respecting the conditions of animal life; and it is not so absolutely impossible as it might at first sight appear to be, that the *Globigerinæ* of the Atlantic sea-bottom do not live and die where they are found.

As I have mentioned, the soundings from the great Atlantic plain are almost entirely made up of *Globigerinæ*, with the granules which have been mentioned, and some few other calcareous shells; but a small percentage of the chalky mud—perhaps at most some five per cent of it—is of a different nature, and consists of shells and skeletons composed of silex, or pure flint. These siliceous bodies belong partly to the lowly vegetable organisms which are called *Diatomaceæ*, and partly to the minute and extremely simple animals, termed *Radiolaria*. It is quite certain that these creatures do not live at the bottom of the ocean, but at its surface—where they may be obtained in prodigious numbers by the use of a properly constructed net. Hence it follows that these siliceous organisms, though they are not heavier than the lightest dust, must have fallen, in some cases, through fifteen thousand feet of water, before they reached their final resting-place on the ocean floor. And, considering how large a surface these bodies expose in proportion to their weight, it is probable that they occupy a great length of time in making their burial journey from the surface of the Atlantic to the bottom.

But if the *Radiolaria* and Diatoms are thus rained upon the bottom of the sea, from the superficial layer of its waters in which they pass their lives, it is obviously possible that the *Globigerinæ* may be similarly derived; and if they were so, it would be much more easy to understand how they obtain their supply of food than it is at present. Nevertheless, the positive and negative evidence all points the other way. The skeletons of the full-grown, deep-sea *Globigerinæ* are so remarkably solid and heavy in proportion to their surface as to seem little fitted for floating; and, as a matter of fact, they are not to be found along with the Diatoms and *Radiolaria*, in the uppermost stratum of the open ocean.

It has been observed, again, that the abundance of *Globigerinæ*, in proportion to other organisms of like kind, increases with the depth of the sea; and that deep-water *Globigerinæ* are larger than

those which live in shallower parts of the sea; and such facts negative the supposition that these organisms have been swept by currents from the shallows into the deeps of the Atlantic.

It therefore seems to be hardly doubtful that these wonderful creatures live and die at the depths in which they are found.*

However, the important points for us are, that the living *Globigerinæ* are exclusively marine animals, the skeletons of which abound at the bottom of deep seas; and that there is not a shadow of reason for believing that the habits of the *Globigerinæ* of the chalk differed from those of the existing species. But if this be true, there is no escaping the conclusion that the chalk itself is the dried mud of an ancient deep sea.

In working over the soundings collected by Captain Dayman, I was surprised to find that many of what I have called the "granules" of that mud were not, as one might have been tempted to think at first, the mere powder and waste of *Globigerinæ*, but that they had a definite form and size. I termed these bodies "*coccoliths*," and doubted their organic nature. Dr. Wallich verified my observation, and added the interesting discovery that, not unfrequently, bodies similar to these "coccoliths" were aggregated together into spheroids, which he termed "*coccospheres*." So far as we knew, these bodies, the nature of which is extremely puzzling and problematical, were peculiar to the Atlantic soundings.

But, a few years ago, Mr. Sorby, in making a careful examination of the chalk by means of thin sections and otherwise, observed, as Ehrenberg had done before him, that much of its granular basis possesses a definite form. Comparing these formed particles with those in the Atlantic soundings, he found the two to be identical; and thus proved that the chalk, like the soundings, contains these mysterious coccoliths and coccospheres. Here was a further and a most interesting confirmation, from internal evidence, of the essential identity of the chalk with modern deep-sea mud. *Globigerinæ*, coccoliths, and coccospheres are found as the chief constituents of both, and testify to the general similarity of the conditions under which both have been formed.*

The evidence furnished by the hewing, facing, and superposition of the stones of the Pyramids, that these structures were built by men, has no greater weight than the evidence that the chalk was built by *Globigerinæ*; and the belief that those ancient pyramid-builders were terrestrial and air-breathing creatures like ourselves, is not better based than the conviction that the chalk-makers lived in the sea.

But as our belief in the building of the Pyramids by men is not only grounded on the internal evidence afforded by these structures, but gathers strength from multitudinous collateral proofs, and is clinched by the total absence of any reason for a contrary belief; so the evidence drawn from the *Globigerinæ* that the chalk is an ancient sea-bottom, is fortified by innumerable independent lines of evidence; and our belief in the truth of the conclusion to which all positive testimony tends, receives the like negative justification from the fact that no other hypothesis has a shadow of foundation.

It may be worth while briefly to consider a few of these collateral proofs that the chalk was deposited at the bottom of the sea.

The great mass of the chalk is composed, as we have seen, of the skeletons

* During the cruise of H.M.S. Bull-dog, commanded by Sir Leopold M'Clintock, in 1860, living star-fish were brought up, clinging to the lowest part of the sounding-line, from a depth of 1260 fathoms, midway between Cape Farewell, in Greenland, and the Rockall banks. Dr. Wallich ascertained that the sea-bottom at this point consisted of the ordinary *Globigerina* ooze, and that the stomachs of the star-fishes were full of *Globigerinæ*. This discovery removes all objections to the existence of living *Globigerinæ* at great depths, which are based upon the supposed difficulty of maintaining animal life under such conditions; and it throws the burden of proof upon those who object to the supposition that the *Globigerinæ* live and die where they are found.

* I have recently traced out the development of the "coccoliths" from a diameter of $\frac{1}{7000}$th of an inch up to their largest size (which is about $\frac{1}{1000}$th), and no longer doubt that they are produced by independent organisms, which, like the *Globigerinæ*, live and die at the bottom of the sea.

of *Globigerinæ*, and other simple organisms, imbedded in granular matter. Here and there, however, this hardened mud of the ancient sea reveals the remains of higher animals which have lived and died, and left their hard parts in the mud, just as the oysters die and leave their shells behind them, in the mud of the present seas.

There are, at the present day, certain groups of animals which are never found in fresh waters, being unable to live anywhere but in the sea. Such are the corals; those corallines which are called *Polyzoa*; those creatures which fabricate the lamp-shells, and are called *Brachiopoda*; the pearly *Nautilus*, and all animals allied to it; and all the forms of sea-urchins and star-fishes.

Not only are all these creatures confined to salt water at the present day; but, so far as our records of the past go, the conditions of their existence have been the same: hence, their occurrence in any deposit is as strong evidence as can be obtained, that that deposit was formed in the sea. Now the remains of animals of all the kinds which have been enumerated occur in the chalk, in greater or less abundance; while not one of those forms of shell-fish which are characteristic of fresh water has yet been observed in it.

When we consider that the remains of more than three thousand distinct species of aquatic animals have been discovered among the fossils of the chalk, that the great majority of them are of such forms as are now met with only in the sea, and that there is no reason to believe that any one of them inhabited fresh water—the collateral evidence that the chalk represents an ancient sea-bottom acquires as great force as the proof derived from the nature of the chalk itself. I think you will now allow that I did not overstate my case when I asserted that we have as strong grounds for believing that all the vast area of dry land, at present occupied by the chalk, was once at the bottom of the sea, as we have for any matter of history whatever; while there is no justification for any other belief.

No less certain it is that the time during which the countries we now call south-east England, France, Germany, Poland, Russia, Egypt, Arabia, Syria, were more or less completely covered by a deep sea, was of considerable duration.

We have already seen that the chalk is, in places, more than a thousand feet thick. I think you will agree with me, that it must have taken some time for the skeletons of animalcules of a hundredth of an inch in diameter to heap up such a mass as that. I have said that throughout the thickness of the chalk the remains of other animals are scattered. These remains are often in the most exquisite state of preservation. The valves of the shell-fishes are commonly adherent; the long spines of some of the sea-urchins, which would be detached by the smallest jar, often remain in their places. In a word, it is certain that these animals have lived and died when the place which they now occupy was the surface of as much of the chalk as had then been deposited; and that each has been covered up by the layer of *Globigerina* mud, upon which the creatures imbedded a little higher up have, in like manner, lived and died. But some of these remains prove the existence of reptiles of vast size in the chalk sea. These lived their time, and had their ancestors and descendants, which assuredly implies time, reptiles being of slow growth.

There is more curious evidence, again, that the process of covering up, or, in other words, the deposit of *Globigerina* skeletons, did not go on very fast. It is demonstrable that an animal of the cretaceous sea might die, that its skeleton might lie uncovered upon the sea-bottom long enough to lose all its outward coverings and appendages by putrefaction; and that, after this had happened, another animal might attach itself to the dead and naked skeleton, might grow to maturity, and might itself die before the calcareous mud had buried the whole.

Cases of this kind are admirably described by Sir Charles Lyell. He speaks of the frequency with which geologists find in the chalk a fossilized sea-urchin, to which is attached the lower valve of a *Crania*. This is a kind of shell-fish, with a shell composed of two pieces, of which, as in the oyster, one is fixed and the other free.

"The upper valve is almost invariably wanting, though occasionally found in a perfect state of preservation in the white chalk at some distance. In this case, we

see clearly that the sea-urchin first lived from youth to age, then died and lost its spines, which were carried away. Then the young *Crania* adhered to the bared shell, grew and perished in its turn; after which, the upper valve was separated from the lower, before the Echinus became enveloped in chalky mud." *

A specimen in the Museum of Practical Geology, in London, still further prolongs the period which must have elapsed between the death of the sea-urchin and its burial by the *Globigerinæ*. For the outward face of the valve of a *Crania*, which is attached to a sea-urchin (*Micraster*), is itself overrun by an incrusting coralline, which spreads thence over more or less of the surface of the sea-urchin. It follows that, after the upper valve of the *Crania* fell off, the surface of the attached valve must have remained exposed long enough to allow of the growth of the whole coralline, since corallines do not live imbedded in the mud.

The progress of knowledge may, one day, enable us to deduce from such facts as these the maximum rate at which the chalk can have accumulated, and thus to arrive at the minimum duration of the chalk period. Suppose that the valve of the *Crania* upon which a coralline has fixed itself in the way just described is so attached to the sea-urchin that no part of it is more than an inch above the face upon which the sea-urchin rests. Then, as the coralline could not have fixed itself, if the *Crania* had been covered up with chalk mud, and could not have lived had itself been so covered, it follows, that an inch of chalk mud could not have accumulated within the time between the death and decay of the soft parts of the sea-urchin and the growth of the coralline to the full size which it has attained. If the decay of the soft parts of the sea-urchin; the attachment, growth to maturity, and decay of the *Crania*; and the subsequent attachment and growth of the coralline, took a year (which is a low estimate enough), the accumulation of the inch of chalk must have taken more than a year: and the deposit of a thousand feet of chalk must, consequently, have taken more than twelve thousand years.

The foundation of all this calculation is, of course, a knowledge of the length of time the *Crania* and the coralline needed to attain their full size; and, on this head, precise knowledge is at present wanting. But there are circumstances which tend to show that nothing like an inch of chalk has accumulated during the life of a *Crania*; and, on any probable estimate of the length of that life, the chalk period must have had a much longer duration than that thus roughly assigned to it.

Thus, not only is it certain that the chalk is the mud of an ancient sea-bottom; but it is no less certain that the chalk sea existed during an extremely long period, though we may not be prepared to give a precise estimate of the length of that period in years. The relative duration is clear, though the absolute duration may not be definable. The attempt to affix any precise date to the period at which the chalk sea began or ended its existence, is baffled by difficulties of the same kind. But the relative age of the cretaceous epoch may be determined with as great ease and certainty as the long duration of that epoch.

You will have heard of the interesting discoveries recently made, in various parts of Western Europe, of flint implements, obviously worked into shape by human hands, under circumstances which show conclusively that man is a very ancient denizen of these regions.

It has been proved that the old populations of Europe, whose existence has been revealed to us in this way, consisted of savages, such as the Esquimaux are now; that, in the country which is now France, they hunted the reindeer, and were familiar with the ways of the mammoth and the bison. The physical geography of France was in those days different from what it is now—the river Somme, for instance, having cut its bed a hundred feet deeper between that time and this; and it is probable that the climate was more like that of Canada or Siberia than that of Western Europe.

The existence of these people is forgotten even in the traditions of the oldest historical nations. The name and

* "Elements of Geology," by Sir Charles Lyell, Bart., F.R.S., p. 23.

fame of them had utterly vanished until a few years back; and the amount of physical change which has been effected since their day renders it more than probable that, venerable as are some of the historical nations, the workers of the chipped flints of Hoxne or of Amiens are to them, as they are to us, in point of antiquity.

But, if we assign to these hoar relics of long-vanished generations of men the greatest age that can possibly be claimed for them, they are not older than the drift, or boulder clay, which, in comparison with the chalk, is but a very juvenile deposit. You need go no further than your own sea-board for evidence of this fact. At one of the most charming spots on the coast of Norfolk, Cromer, you will see the boulder clay forming a vast mass, which lies upon the chalk, and must consequently have come into existence after it. Huge boulders of chalk are, in fact, included in the clay, and have evidently been brought to the position they now occupy by the same agency as that which has planted blocks of syenite from Norway side by side with them.

The chalk, then, is certainly older than the boulder clay. If you ask how much, I will again take you no further than the same spot upon your own coasts for evidence. I have spoken of the boulder clay and drift as resting upon the chalk. That is not strictly true. Interposed between the chalk and the drift is a comparatively insignificant layer, containing vegetable matter. But that layer tells a wonderful history. It is full of stumps of trees standing as they grew. Fir-trees are there with their cones, and hazel-bushes with their nuts; there stand the stools of oak and yew trees, beeches and alders. Hence this stratum is appropriately called the "forest-bed."

It is obvious that the chalk must have been upheaved and converted into dry land before the timber trees could grow upon it. As the bolls of some of these trees are from two to three feet in diameter, it is no less clear that the dry land thus formed remained in the same condition for long ages. And not only do the remains of stately oaks and well-grown firs testify to the duration of this condition of things, but additional evidence to the same effect is afforded by the abundant remains of elephants, rhinoceroses, hippopotamuses, and other great wild beasts, which it has yielded to the zealous search of such men as the Rev. Mr. Gunn.

When you look at such a collection as he has formed, and bethink you that these elephantine bones did veritably carry their owners about, and these great grinders crunch, in the dark woods of which the forest-bed is now the only trace, it is impossible not to feel that they are as good evidence of the lapse of time as the annual rings of the tree-stumps.

Thus there is a writing upon the wall of cliffs at Cromer, and whoso runs may read it. It tells us, with an authority which cannot be impeached that the ancient sea-bed of the chalk sea was raised up, and remained dry land, until it was covered with forest, stocked with the great game whose spoils have rejoiced your geologists. How long it remained in that condition cannot be said; but "the whirligig of time brought its revenges" in those days as in these. That dry land, with the bones and teeth of generations of long-lived elephants, hidden away among the gnarled roots and dry leaves of its ancient trees, sank gradually to the bottom of the icy sea, which covered it with huge masses of drift and boulder clay. Sea-beasts, such as the walrus, now restricted to the extreme north, paddled about where birds had twittered among the topmost twigs of the fir-trees. How long this state of things endured we know not, but at length it came to an end. The upheaved glacial mud hardened into the soil of modern Norfolk. Forests grew once more, the wolf and the beaver replaced the reindeer and the elephant; and at length what we call the history of England dawned.

Thus you have, within the limits of your own county, proof that the chalk can justly claim a very much greater antiquity than even the oldest physical traces of mankind. But we may go further and demonstrate, by evidence of the same authority as that which testifies to the existence of the father of men, that the chalk is vastly older than Adam himself.

The Book of Genesis informs us that Adam, immediately upon his creation, and before the appearance of Eve, was

placed in the garden of Eden. The problem of the geographical position of Eden has greatly vexed the spirits of the learned in such matters, but there is one point respecting which, so far as I know, no commentator has ever raised a doubt. This is, that of the four rivers which are said to run out of it, Euphrates and Hiddekel are identical with the rivers now known by the names of Euphrates and Tigris.

But the whole country in which these mighty rivers take their origin, and through which they run, is composed of rocks which are either of the same age as the chalk, or of later date. So that the chalk must not only have been formed, but, after its formation, the time required for the deposit of these later rocks, and for their upheaval into dry land, must have elapsed, before the smallest brook which feeds the swift stream of "the great river, the river of Babylon," began to flow.

Thus, evidence which cannot be rebutted, and which need not be strengthened, though if time permitted I might indefinitely increase its quantity, compels you to believe that the earth, from the time of the chalk to the present day, has been the theatre of a series of changes as vast in their amount as they were slow in their progress. The area on which we stand has been first sea and then land, for at least four alternations; and has remained in each of these conditions for a period of great length.

Nor have these wonderful metamorphoses of sea into land, and of land into sea, been confined to one corner of England. During the chalk period, or "cretaceous epoch," not one of the present great physical features of the globe was in existence. Our great mountain ranges, Pyrenees, Alps, Himalayas, Andes, have all been upheaved since the chalk was deposited, and the cretaceous sea flowed over the sites of Sinai and Ararat.

All this is certain, because rocks of cretaceous or still later date have shared in the elevatory movements which gave rise to these mountain chains; and may be found perched up, in some cases, many thousand feet high upon their flanks. And evidence of equal cogency demonstrates that, though in Norfolk the forest-bed rests directly upon the chalk, yet it does so, not because the period at which the forest grew immediately followed that at which the chalk was formed, but because an immense lapse of time, represented elsewhere by thousands of feet of rock, is not indicated at Cromer.

I must ask you to believe that there is no less conclusive proof that a still more prolonged succession of similar changes occurred, before the chalk was deposited. Nor have we any reason to think that the first term in the series of these changes is known. The oldest sea-beds preserved to us are sands, and mud, and pebbles, the wear and tear of rocks which were formed in still older oceans.

But, great as is the magnitude of these physical changes of the world, they have been accompanied by a no less striking series of modifications in its living inhabitants.

All the great classes of animals, beasts of the field, fowls of the air, creeping things, and things which dwell in the waters, flourished upon the globe long ages before the chalk was deposited. Very few, however, if any, of these ancient forms of animal life were identical with those which now live. Certainly not one of the higher animals was of the same species as any of those now in existence. The beasts of the field, in the days before the chalk, were not our beasts of the field, nor the fowls of the air such as those which the eye of men has seen flying, unless his antiquity dates infinitely further back than we at present surmise. If we could be carried back into those times, we should be as one suddenly set down in Australia before it was colonized. We should see mammals, birds, reptiles, fishes, insects, snails, and the like, clearly recognizable as such, and yet not one of them would be just the same as those with which we are familiar, and many would be extremely different.

From that time to the present, the population of the world has undergone slow and gradual, but incessant, changes. There has been no grand catastrophe—no destroyer has swept away the forms of life of one period, and replaced them by a totally new creation; but one species has vanished and another has taken

its place; creatures of one type of structure have diminished, those of another have increased, as time has passed on. And thus, while the differences between the living creatures of the time before the chalk and those of the present day appear startling, if placed side by side, we are led from one to the other by the most gradual progress, if we follow the course of Nature through the whole series of those relics of her operations which she has left behind.

And it is by the population of the chalk sea that the ancient and the modern inhabitants of the world are most completely connected. The groups which are dying out flourish, side by side, with the groups which are now the dominant forms of life.

Thus the chalk contains remains of these strange flying and swimming reptiles, the pterodactyl, the ichthyosaurus, and the plesiosaurus, which are found in no later deposits, but abounded in preceding ages. The chambered shells called ammonites and belemnites, which are so characteristic of the period preceding the cretaceous, in like manner die with it.

But, among these fading remainders of a previous state of things, are some very modern forms of life, looking like Yankee peddlers among a tribe of red Indians. Crocodiles of modern type appear; bony fishes, many of them very similar to existing species, almost supplant the forms of fish which predominate in more ancient seas; and many kinds of living shell-fish first become known to us in the chalk. The vegetation acquires a modern aspect. A few living animals are not even distinguishable as species from those which existed at that remote epoch. The *Globigerina* of the present day, for example, is not different specifically from that of the chalk; and the same may be said of many other *Foraminifera*. I think it probable that critical and unprejudiced examination will show that more than one species of much higher animals have had a similar longevity; but the only example which I can at present give confidently is the snake's-head lamp-shell (*Terebratulina caput serpentis*), which lives in our English seas and abounded (as *Terebratulina striata* of authors) in the chalk.

The longest line of human ancestry must hide its diminished head before the pedigree of this insignificant shell-fish. We Englishmen are proud to have an ancestor who was present at the Battle of Hastings. The ancestors of *Terebratulina caput serpentis* may have been present at a battle of *Ichthyosauria* in that part of the sea which, when the chalk was forming, flowed over the site of Hastings. While all around has changed, this *Terebratulina* has peacefully propagated its species from generation to generation, and stands to this day as a living testimony to the continuity of the present with the past history of the globe.

Up to this moment I have stated, so far as I know, nothing but well-authenticated facts, and the immediate conclusions which they force upon the mind.

But the mind is so constituted that it does not willingly rest in facts and immediate causes, but seeks always after a knowledge of the remoter links in the chain of causation.

Taking the many changes of any given spot of the earth's surface, from sea to land and from land to sea, as an established fact, we cannot refrain from asking ourselves how these changes have occurred. And when we have explained them—as they must be explained—by the alternate slow movements of elevation and depression which have affected the crust of the earth, we go still further back, and ask, Why these movements?

I am not certain that any one can give you a satisfactory answer to that question. Assuredly I cannot. All that can be said for certain is, that such movements are part of the ordinary course of nature, inasmuch as they are going on at the present time. Direct proof may be given, that some parts of the land of the northern hemisphere are at this moment insensibly rising and others insensibly sinking; and there is indirect but perfectly satisfactory proof, that an enormous area now covered by the Pacific has been deepened thousands of feet since the present inhabitants of that sea came into existence.

Thus there is not a shadow of a reason for believing that the physical changes of the globe, in past times, have been

effected by other than natural causes.

Is there any more reason for believing that the concomitant modifications in the forms of the living inhabitants of the globe have been brought about in other ways?

Before attempting to answer this question, let us try to form a distinct mental picture of what has happened in some special case.

The crocodiles are animals which, as a group, have a very vast antiquity. They abounded ages before the chalk was deposited; they throng the rivers in warm climates, at the present day. There is a difference in the form of the joints of the backbone, and in some minor particulars, between the crocodiles of the present epoch and those which lived before the chalk; but, in the cretaceous epoch, as I have already mentioned, the crocodiles had assumed the modern type of structure. Notwithstanding this, the crocodiles of the chalk are not identically the same as those which lived in the times called "older tertiary," which succeeded the cretaceous epoch; and the crocodiles of the older tertiaries are not identical with those of the newer tertiaries, nor are these identical with existing forms. I leave open the question whether particular species may have lived on from epoch to epoch. But each epoch has had its peculiar crocodiles; though all, since the chalk, have belonged to the modern type, and differ simply in their proportions and in such structural particulars as are discernible only to trained eyes.

How is the existence of this long succession of different species of crocodiles to be accounted for?

Only two suppositions seem to be open to us—Either each species of crocodile has been specially created, or it has arisen out of some pre-existing form by the operation of natural causes.

Choose your hypothesis; I have chosen mine. I can find no warranty for believing in the distinct creation of a score of successive species of crocodiles in the course of countless ages of time. Science gives no countenance to such a wild fancy; nor can even the perverse ingenuity of a commentator pretend to discover this sense, in the simple words in which the writer of Genesis records the proceedings of the fifth and sixth days of the Creation.

On the other hand, I see no good reason for doubting the necessary alternative, that all these varied species have been evolved from pre-existing crocodilian forms by the operation of causes as completely a part of the common order of nature as those which have effected the changes of the inorganic world.

Few will venture to affirm that the reasoning which applies to crocodiles loses its force among other animals or among plants. If one series of species has come into existence by the operation of natural causes, it seems folly to deny that all may have arisen in the same way.

A small beginning has led us to a great ending. If I were to put the bit of chalk with which we started into the hot but obscure flame of burning hydrogen, it would presently shine like the sun. It seems to me that this physical metamorphosis is no false image of what has been the result of our subjecting it to a jet of fervent, though nowise brilliant, thought to-night. It has become luminous, and its clear rays, penetrating the abyss of the remote past, have brought within our ken some stages of the evolution of the earth. And in the shifting "without haste, but without rest" of the land and sea, as in the endless variation of the forms assumed by living beings, we have observed nothing but the natural product of the forces originally possessed by the substance of the universe.

GEOLOGICAL CONTEMPORANEITY AND PERSISTENT TYPES OF LIFE.

Merchants occasionally go through a wholesome, though troublesome and not always satisfactory, process which they term "taking stock." After all the excitement of speculation, the pleasure of gain, and the pain of loss, the trader makes up his mind to face facts and to learn the exact quantity and quality of his solid and reliable possessions.

The man of science does well sometimes to imitate this procedure; and, forgetting for the time the importance of his own small winnings, to re-examine the common stock in trade, so that he may make sure how far the stock of bullion in

the cellar—on the faith of whose existence so much paper has been circulating—is really the solid gold of truth.

The Anniversary Meeting of the Geological Society seems to be an occasion well suited for an undertaking of this kind—for an inquiry, in fact, into the nature and value of the present results of palæontological investigation; and the more so, as all those who have paid close attention to the late multitudinous discussions in which palæontology is implicated, must have felt the urgent necessity of some such scrutiny.

First in order, as the most definite and unquestionable of all the results of palæontology, must be mentioned the immense extension and impulse given to botany, zoology, and comparative anatomy, by the investigation of fossil remains. Indeed, the mass of biological facts has been so greatly increased, and the range of biological speculation has been so vastly widened, by the researches of the geologist and palæontologist, that it is to be feared there are naturalists in existence who look upon geology as Brindley regarded rivers. "Rivers," said the great engineer, "were made to feed canals;" and geology, some seem to think, was solely created to advance comparative anatomy.

Were such a thought justifiable, it could hardly expect to be received with favor by this assembly. But it is not justifiable. Your favorite science has her own great aims independent of all others; and if, notwithstanding her steady devotion to her own progress, she can scatter such rich alms among her sisters, it should be remembered that her charity is of the sort that does not impoverish, but "blesseth him that gives and him that takes."

Regard the matter as we will, however, the facts remain. Nearly 40,000 species of animals and plants have been added to the Systema Naturæ by palæontological research. This is a living population equivalent to that of a new continent in mere number; equivalent to that of a new hemisphere, if we take into account the small population of insects as yet found fossil, and the large proportion and peculiar organization of many of the Vertebrata.

But, beyond this, it is perhaps not too much to say that, except for the necessity of interpreting palæontological facts, the laws of distribution would have received less careful study; while few comparative anatomists (and those not of the first order) would have been induced by mere love of detail, as such, to study the minutiæ of osteology, were it not that in such minutiæ lie the only keys to the most interesting riddles offered by the extinct animal world.

These assuredly are great and solid gains. Surely it is matter for no small congratulation that in half a century (for palæontology, though it dawned earlier, came into full day only with Cuvier) a subordinate branch of biology should have doubled the value and the interest of the whole group of sciences to which it belongs.

But this is not all. Allied with geology, palæontology has established two laws of inestimable importance: the first, that one and the same area of the earth's surface has been successively occupied by very different kinds of living beings; the second, that the order of succession established in one locality holds good, approximately, in all.

The first of these laws is universal and irreversible; the second is an induction from a vast number of observations, though it may possibly, and even probably, have to admit of exceptions. As a consequence of the second law, it follows that a peculiar relation frequently subsists between series of strata, containing organic remains, in different localities. The series resemble one another, not only in virtue of a general resemblance of the organic remains in the two, but also in virtue of a resemblance in the order and character of the serial succession in each. There is a resemblance of arrangement; so that the separate terms of each series, as well as the whole series, exhibit a correspondence.

Succession implies time; the lower members of a series of sedimentary rocks are certainly older than the upper; and when the notion of age was once introduced as the equivalent of succession, it was no wonder that correspondence in succession came to be looked upon as correspondence in age, or "contemporaneity." And, indeed, so long as relative age only is spoken of, correspond-

ence in succession *is* correspondence in age; it is *relative* contemporaneity.

But it would have been very much better for geology if so loose and ambiguous a word as "contemporaneous" had been excluded from her terminology, and if, in its stead, some term expressing similarity of serial relation, and excluding the notion of time altogether, had been employed to denote correspondence in position in two or more series of strata.

In anatomy, where such correspondence of position has constantly to be spoken of, it is denoted by the word "homology" and its derivatives; and for Geology (which after all is only the anatomy and physiology of the earth) it might be well to invent some single word, such as "homotaxis" (similarity of order), in order to express an essentially similar idea. This, however, has not been done, and most probably the inquiry will at once be made—To what end burden science with a new and strange term in place of one old, familiar, and part of our common language?

The reply to this question will become obvious as the inquiry into the results of palæontology is pushed further.

Those whose business it is to acquaint themselves specially with the works of palæontologists, in fact, will be fully aware that very few, if any, would rest satisfied with such a statement of the conclusions of their branch of biology as that which has just been given.

Our standard repertories of palæontology profess to teach us far higher things—to disclose the entire succession of living forms upon the surface of the globe; to tell us of a wholly different distribution of climatic conditions in ancient times; to reveal the character of the first of all living existences; and to trace out the law of progress from them to us.

It may not be unprofitable to bestow on these professions a somewhat more critical examination than they have hitherto received, in order to ascertain how far they rest on an irrefragable basis; or whether, after all, it might not be well for palæontologists to learn a little more carefully that scientific "ars artium," the art of saying "I don't know." And to this end let us define somewhat more exactly the extent of these pretensions of palæontology.

Every one is aware that Professor Bronn's "Untersuchungen" and Professor Pictet's "Traité de Paléontologie" are works of standard authority, familiarly consulted by every working palæontologist. It is desirable to speak of these excellent books, and of their distinguished authors, with the utmost respect, and in a tone as far as possible removed from carping criticism; indeed, if they are specially cited in this place, it is merely in justification of the assertion that the following propositions, which may be found implicitly or explicitly in the works in question, are regarded by the mass of palæontologists and geologists, not only on the Continent but in this country, as expressing some of the best-established results of palæontology. Thus:

Animals and plants began their existence together, not long after the commencement of the deposition of the sedimentary rocks; and then succeeded one another, in such a manner, that totally distinct faunæ and floræ occupied the whole surface of the earth, one after the other, and during distinct epochs of time.

A geological formation is the sum of all the strata deposited over the whole surface of the earth during one of these epochs: a geological fauna or flora is the sum of all the species of animals or plants which occupied the whole surface of the globe, during one of these epochs.

The population of the earth's surface was at first very similar in all parts, and only from the middle of the Tertiary epoch onward, began to show a distinct distribution in zones.

The constitution of the original population, as well as the numerical proportions of its members, indicates a warmer and, on the whole, somewhat tropical climate, which remained tolerably equable throughout the year. The subsequent distribution of living beings in zones is the result of a gradual lowering of the general temperature, which first began to be felt at the poles.

It is not now proposed to inquire whether these doctrines are true or false; but to direct your attention to a much simpler though very essential preliminary question—What is their logical basis?

what are the fundamental assumptions upon which they all logically depend? and what is the evidence on which those fundamental propositions demand our assent?

These assumptions are two: the first, that the commencement of the geological record is coeval with the commencement of life on the globe; the second, that geological contemporaneity is the same thing as chronological synchrony. Without the first of these assumptions there would of course be no ground for any statement respecting the commencement of life; without the second, all the other statements cited, every one of which implies a knowedge of the state of different parts of the earth at one and the same time, will be no less devoid of demonstration.

The first assumption obviously rests entirely on negative evidence. This is, of course, the only evidence that ever can be available to prove the commencement of any series of phenomena; but, at the same time, it must be recollected that the value of negative evidence depends entirely on the amount of positive corroboration it receives. If A. B. wishes to prove an *alibi*, it is of no use for him to get a thousand witnesses simply to swear that they did not see him in such and such a place, unless the witnesses are prepared to prove that they must have seen him had he been there. But the evidence that animal life commenced with the Lingula-flags, *e.g.*, would seem to be exactly of this unsatisfactory uncorroborated sort. The Cambrian witnesses simply swear they "haven't seen anybody their way;" upon which the counsel for the other side immediately puts in ten or twelve thousand feet of Devonian sandstones to make oath they never saw a fish or a mollusk, though all the world knows there were plenty in their time.

But then it is urged that, though the Devonian rocks in one part of the world exhibit no fossils, in another they do, while the lower Cambrian rocks nowhere exhibit fossils, and hence no living being could have existed in their epoch.

To this there are two replies: the first, that the observational basis of the assertion that the lowest rocks are nowhere fossiliferous is an amazingly small one, seeing how very small an area, in comparison to that of the whole world, has yet been fully searched; the second, that the argument is good for nothing unless the unfossiliferous rocks in question were not only *contemporaneous* in the geological sense, but *synchronous* in the chronological sense. To use the *alibi* illustration again. If a man wishes to prove he was in neither of two places, A and B, on a given day, his witnesses for each place must be prepared to answer for the whole day. If they can only prove that he was not at A in the morning, and not at B in the afternoon, the evidence of his absence from both is *nil*, because he might have been at B in the morning and at A in the afternoon.

Thus everything depends upon the validity of the second assumption. And we must proceed to inquire what is the real meaning of the word "contemporaneous" as employed by geologists. To this end a concrete example may be taken.

The Lias of England and the Lias of Germany, the Cretaceous rocks of Britain and the Cretaceous rocks of Southern India, are termed by geologists "contemporaneous" formations; but whenever any thoughtful geologist is asked whether he means to say that they were deposited synchronously, he says, "No—only within the same great epoch." And if, in pursuing the inquiry, he is asked what may be the approximate value in time of a "great epoch"—whether it means a hundred years, or a thousand, or a million, or ten million years—his reply is, "I cannot tell."

If the further question be put, whether physical geology is in possession of any method by which the actual synchrony (or the reverse) of any two distant deposits can be ascertained, no such method can be heard of; it being admitted by all the best authorities that neither similarity of mineral composition, nor of physical character, nor even direct continuity of stratum, are *absolute* proofs of the synchronism of even approximated sedimentary strata: while, for distant deposits, there seems to be no kind of physical evidence attainable of a nature competent to decide whether such deposits were formed simultaneously, or whether they possess any given difference of antiquity. To return to an example already

given. All competent authorities will probably assent to the proposition that physical geology does not enable us in any way to reply to this question—Were the British Cretaceous rocks deposited at the same time as those of India, or are they a million of years younger or a million of years older?

Is palæontology able to succeed where physical geology fails? Standard writers on palæontology, as has been seen, assume that she can. They take it for granted, that deposits containing similar organic remains are synchronous—at any rate in a broad sense; and yet, those who will study the eleventh and twelfth chapters of Sir Henry De la Beche's remarkable "Researches in Theoretical Geology," published now nearly thirty years ago, and will carry out the arguments there most luminously stated, to their logical consequences, may very easily convince themselves that even absolute identity of organic contents is no proof of the synchrony of deposits, while absolute diversity is no proof of difference of date. Sir Henry De la Beche goes even further, and adduces conclusive evidence to show that the different parts of one and the same stratum, having a similar composition throughout, containing the same organic remains, and having similar beds above and below it, may yet differ to any conceivable extent in age.

Edward Forbes was in the habit of asserting that the similarity of the organic contents of distant formations was *primâ facie* evidence, not of their similarity, but of their difference of age; and holding as he did the doctrine of single specific centres, the conclusion was as legitimate as any other; for the two districts must have been occupied by migration from one of the two, or from an intermediate spot, and the chances against exact coincidence of migration and of imbedding are infinite.

In point of fact, however, whether the hypothesis of single or of multiple specific centres be adopted, similarity of organic contents cannot possibly afford any proof of the synchrony of the deposits which contain them; on the contrary, it is demonstrably compatible with the lapse of the most prodigious intervals of time, and with interposition of vast changes in the organic and inorganic worlds, between the epochs in which such deposits were formed.

On what amount of similarity of their faunæ is the doctrine of the contemporaneity of the European and of the North American Silurians based? In the last edition of Sir Charles Lyell's "Elementary Geology" it is stated, on the authority of a former president of this society, the late Daniel Sharpe, that between 30 and 40 per cent of the species of Silurian Mollusca are common to both sides of the Atlantic. By way of due allowance for further discovery, let us double the lesser number and suppose that 60 per cent of the species are common to the North American and the British Silurians. Sixty per cent of species in common is, then, proof of contemporaneity.

Now suppose that, a million or two of years hence, when Britain has made another dip beneath the sea and has come up again, some geologist applies this doctrine, in comparing the strata laid bare by the upheaval of the bottom, say, of St. George's Channel with what may then remain of the Suffolk Crag. Reasoning in the same way, he will at once decide the Suffolk Crag and the St. George's Channel beds to be contemporaneous; although we happen to know that a vast period (even in the geological sense) of time, and physical changes of almost unprecedented extent, separate the two.

But if it be a demonstrable fact that strata containing more than 60 or 70 per cent of species of Mollusca in common, and comparatively close together, may yet be separated by an amount of geological time sufficient to allow of some of the greatest physical changes the world has seen, what becomes of that sort of contemporaneity, the sole evidence of which is a similarity of facies, or the identity of half a dozen species, or of a good many genera?

And yet there is no better evidence for the contemporaneity assumed by all who adopt the hypotheses of universal faunæ and floræ, of a universally uniform climate, and of a sensible cooling of the globe during geological time.

There seems, then, no escape from the admission that neither physical geology

nor palæontology possesses any method by which the absolute synchronism of two strata can be demonstrated. All that geology can prove is local order of succession. It is mathematically certain that, in any given vertical linear section of an undisturbed series of sedimentary deposits, the bed which lies lowest is the oldest. In any other vertical linear section of the same series, of course, corresponding beds will occur in a similar order; but, however great may be the probability, no man can say with absolute certainty that the beds in the two sections were synchronously deposited. For areas of moderate extent, it is doubtless true that no practical evil is likely to result from assuming the corresponding beds to be synchronous or strictly contemporaneous; and there are multitudes of accessory circumstances which may fully justify the assumption of such synchrony. But the moment the geologist has to deal with large areas, or with completely separated deposits, the mischief of confounding that "homotaxis" or "similarity of arrangement," which *can* be demonstrated, with "synchrony" or "identity of date," for which there is not a shadow of proof, under the one common term of "contemporaneity" becomes incalculable, and proves the constant source of gratuitous speculations.

For anything that geology or palæontology are able to show to the contrary, a Devonian fauna and flora in the British Islands may have been contemporaneous with Silurian life in North America, and with a Carboniferous fauna and flora in Africa. Geographical provinces and zones may have been as distinctly marked in the Palæozoic epoch as at present, and those seemingly sudden appearances of new genera and species, which we ascribe to new creation, may be simple results of migration.

It may be so; it may be otherwise. In the present condition of our knowledge and of our methods, one verdict—"not proven, and not provable"—must be recorded against all the grand hypotheses of the palæontologist respecting the general succession of life on the globe. The order and nature of terrestrial life, as a whole, are open questions. Geology at present provides us with most valuable topographical records, but she has not the means of working them into a universal history. Is such a universal history, then, to be regarded as unattainable? Are all the grandest and most interesting problems which offer themselves to the geological student essentially insoluble? Is he in the position of a scientific Tantalus — doomed always to thirst for a knowledge which he cannot obtain? The reverse is to be hoped; nay, it may not be impossible to indicate the source whence help will come.

In commencing these remarks, mention was made of the great obligations under which the naturalist lies to the geologist and palæontologist. Assuredly the time will come when these obligations will be repaid tenfold, and when the maze of the world's past history, through which the pure geologist and the pure palæontologist find no guidance, will be securely threaded by the clue furnished by the naturalist.

All who are competent to express an opinion on the subject are, at present, agreed that the manifold varieties of animal and vegetable form have not either come into existence by chance, nor result from capricious exertions of creative power; but that they have taken place in a definite order, the statement of which order is what men of science term a natural law. Whether such a law is to be regarded as an expression of the mode of operation of natural forces, or whether it is simply a statement of the manner in which a supernatural power has thought fit to act, is a secondary question, so long as the existence of the law and the possibility of its discovery by the human intellect are granted. But he must be a half-hearted philosopher who, believing in that possibility, and having watched the gigantic strides of the biological sciences during the last twenty years, doubts that science will sooner or later make this further step, so as to become possessed of the law of evolution of organic forms—of the unvarying order of that great chain of causes and effects of which all organic forms, ancient and modern, are the links. And then, if ever, we shall be able to begin to discuss, with profit, the questions respecting the commencement of life, and the nature of the successive populations of the globe, which so many seem to thin[k] [a]re already an-

swered.

The preceding arguments make no particular claim to novelty; indeed, they have been floating more or less distinctly before the minds of geologists for the last thirty years; and if, at the present time, it has seemed desirable to give them more definite and systematic expression, it is because palæontology is every day assuming a greater importance, and now requires to rest on a basis the firmness of which is thoroughly well assured. Among its fundamental conceptions, there must be no confusion between what is certain and what is more or less probable. But, pending the construction of a surer foundation than palæontology now possesses, it may be instructive, assuming for the nonce the general correctness of the ordinary hypothesis of geological contemporaneity, to consider whether the deductions which are ordinarily drawn from the whole body of palæontological facts are justifiable.

The evidence on which such conclusions are based is of two kinds, negative and positive. The value of negative evidence, in connection with this inquiry, has been so fully and clearly discussed in an address from the chair of this society, which none of us have forgotten, that nothing need at present be said about it; the more, as the considerations which have been laid before you have certainly not tended to increase your estimation of such evidence. It will be preferable to turn to the positive facts of palæontology, and to inquire what they tell us.

We are all accustomed to speak of the number and the extent of the changes in the living population of the globe during geological time as something enormous; and indeed they are so, if we regard only the negative differences which separate the older rocks from the more modern, and if we look upon specific and generic changes as great changes, which from one point of view they truly are. But leaving the negative differences out of consideration, and looking only at the positive data furnished by the fossil world from a broader point of view—from that of the comparative anatomist who has made the study of the greater modifications of animal form his chief business—a surprise of another kind dawns upon the mind; and under *this* aspect the smallness of the total change becomes as astonishing as was its greatness under the other.

There are two hundred known orders of plants; of these not one is certainly known to exist exclusively in the fossil state. The whole lapse of geological time has as yet yielded not a single new ordinal type of vegetable structure.*

The positive change in passing from the recent to the ancient animal world is greater, but still singularly small. No fossil animal is so distinct from those now living as to require to be arranged even in a separate class from those which contain existing forms. It is only when we come to the orders, which may be roughly estimated at about a hundred and thirty, that we meet with fossil animals so distinct from those now living as to require orders for themselves; and these do not amount, on the most liberal estimate, to more than about 10 per cent of the whole.

There is no certainly known extinct order of Protozoa; there is but one among the Cœlenterata—that of the rugose corals; there is none among the Mollusca; there are three, the Cystidea, Blastoidea, and Edrioasterida among the Echinoderms; and two, the Trilobita and Eurypterida, among the Crustacea; making altogether five for the great sub-kingdom of Annulosa. Among Vertebrates there is no ordinally distinct fossil fish: there is only one extinct order of Amphibia—the Labyrinthodonts; but there are at least four distinct orders of Reptilia, viz., the Ichthyosauria, Plesiosauria, Pterosauria, Dinosauria, and perhaps another or two. There is no known extinct order of birds, and no certainly known extinct order of Mammals, the ordinal distinctness of the "Toxodontia" being doubtful.

The objection that broad statements of this kind, after all, rest largely on negative evidence is obvious, but it has less force than may at first be supposed; for, as might be expected from the circumstances of the case, we possess more abundant positive evidence regarding Fishes and marine Mollusks than respecting any

* See Hooker's "Introductory Essay to the Flora of Tasmania," p. xxiii.

other forms of animal life; and yet these offer us, through the whole range of geological time, no species ordinally distinct from those now living; while the far less numerous class of Echinoderms presents three, and the Crustacea two, such orders, though none of these come down later than the Palæozoic age. Lastly, the Reptilia present the extraordinary and exceptional phenomenon of as many extinct as existing orders, if not more; the four mentioned maintaining their existence from the Lias to the Chalk inclusive.

Some years ago one of your secretaries pointed out another kind of positive palæontological evidence tending toward the same conclusion—afforded by the existence of what he termed "persistent types" of vegetable and of animal life. He stated, on the authority of Dr. Hooker, that there are Carboniferous plants which appear to be generically identical with some now living; that the cone of the Oolitic *Araucaria* is hardly distinguishable from that of an existing species; that a true *Pinus* appears in the Purbecks, and a *Juglans* in the Chalk; while, from the Bagshot Sands, a *Banksia*, the wood of which is not distinguishable from that of species now living in Australia, had been obtained.

Turning to the animal kingdom, he affirmed the tabulate corals of the Silurian rocks to be wonderfully like those which now exist; while even the families of the Aporosa were all represented in the older Mesozoic rocks.

Among the Mollusca similar facts were adduced. Let it be borne in mind that *Avicula, Mytilus, Chiton, Natica, Patella, Trochus, Discina, Orbicula, Lingula, Rhynchonella*, and *Nautilus*, all of which are existing *genera*, are given without a doubt as Silurian in the last edition of "Siluria;" while the highest forms of the highest Cephalopods are represented in the Lias by a genus, *Belemnoteuthis*, which presents the closest relation to the existing *Loligo*.

The two highest groups of the Annulosa, the Insecta and the Arachnida, are represented in the Coal, either by existing genera, or by forms differing from existing genera in quite minor peculiarities.

Turning to the Vertebrata, the only palæozoic Elasmobranch Fish of which we have any complete knowledge is the Devonian and Carboniferous *Pleuracanthus*, which differs no more from existing Sharks than these do from one another.

Again, vast as is the number of undoubtedly Ganoid fossil Fishes, and great as is their range in time, a large mass of evidence has recently been adduced to show that almost all those respecting which we possess sufficient information, are referable to the same subordinal groups as the existing *Lepidosteus, Polypterus*, and Sturgeon; and that a singular relation obtains between the older and the younger Fishes; the former, the Devonian Ganoids, being almost all members of the same sub-order as *Polypterus*, while the Mesozoic Ganoids are almost all similarly allied to *Lepidosteus*.

Again, what can be more remarkable than the singular constancy of structure preserved throughout a vast period of time by the family of the Pycnodonts and by that of the true Cœlacanths; the former persisting, with but insignificant modifications, from the Carboniferous to the Tertiary rocks, inclusive; the latter existing, with still less change, from the Carboniferous rocks to the Chalk, inclusive?

Among Reptiles, the highest living group, that of the Crocodilia, is represented, at the early part of the Mesozoic epoch, by species identical in the essential characters of their organization with those now living, and differing from the latter only in such matters as the form of the articular facets of the vertebral centra, in the extent to which the nasal passages are separated from the cavity of the mouth by bone, and in the proportions of the limbs.

And even as regards the Mammalia, the scanty remains of Triassic and Oolitic species afford no foundation for the supposition that the organization of the oldest forms differed nearly so much from some of those which now live as these differ from one another.

It is needless to multiply these instances; enough has been said to justify the statement that, in view of the immense diversity of known animal and vegetable forms, and the enormous lapse of time indicated by the accumulation of fossiliferous strata, the only circum-

stance to be wondered at is, not that the changes of life, as exhibited by positive evidence, have been so great, but that they have been so small.

Be they great or small, however, it is desirable to attempt to estimate them. Let us, therefore, take each great division of the animal world in succession, and, whenever an order or a family can be shown to have had a prolonged existence, let us endeavor to ascertain how far the later members of the group differ from the earlier ones. If these later members, in all or in many cases, exhibit a certain amount of modification, the fact is, so far, evidence in favor of a general law of change; and, in a rough way, the rapidity of that change will be measured by the demonstrable amount of modification. On the other hand, it must be recollected that the absence of any modification, while it may leave the doctrine of the existence of a law of change without positive support, cannot possibly disprove all forms of that doctrine, though it may afford a sufficient refutation of many of them.

The PROTOZOA.—The Protozoa are represented throughout the whole range of geological series, from the Lower Silurian formation to the present day. The most ancient forms recently made known by Ehrenberg are exceedingly like those which now exist: no one has ever pretended that the difference between any ancient and any modern Foraminifera is of more than generic value, nor are the oldest Foraminifera either simpler, more embyronic, or less differentiated, than the existing forms.

The CŒLENTERATA.—The Tabulate Corals have existed from the Silurian epoch to the present day, but I am not aware that the ancient *Heliolites* possesses a single mark of a more embryonic or less differentiated character, or less high organization, than the existing *Heliopora*. As for the Aporose Corals, in what respect is the Silurian *Palæocyclus* less highly organized or more embryonic than the modern *Fungia*, or the Liassic Aporosa than the existing members of the same families?

The MOLLUSCA.—In what sense is the living *Waldheimia* less embryonic, or more specialized, than the palæozoic *Spirifer*; or the existing *Rhynchonellæ*, *Craniæ*, *Discinæ*, *Lingulæ*, than the Silurian species of the same genera? In what sense can *Loligo* or *Spirula* be said to be more specialized, or less embryonic, than *Belemnites*; or the modern species of Lamellibranch and Gasteropod genera, than the Silurian species of the same genera?

The ANNULOSA.—The Carboniferous Insecta and Arachnida are neither less specialized, nor more embryonic, than those that now live, nor are the Liassic Cirripedia and Macrura; while several of the Brachyura, which appear in the Chalk, belong to existing genera; and none exhibit either an intermediate or an embryonic character.

The VERTEBRATA.—Among fishes I have referred to the Cœlacanthini (comprising the genera *Cœlacanthus*, *Holophagus*, *Undina*, and *Macropoma*) as affording an example of a persistent type; and it is most remarkable to note the smallness of the differences between any of these fishes (affecting at most the proportions of the body and fins, and the character and sculpture of the scales), notwithstanding their enormous range in time. In all the essentials of its very peculiar structure, the *Macropoma* of the Chalk is identical with the *Cœlacanthus* of the Coal. Look at the genus *Lepidotus*, again, persisting without a modification of importance from the Liassic to the Eocene formations inclusive.

Or among the Teleostei—in what respect is the *Beryx* of the Chalk more embryonic, or less differentiated, than *Beryx lineatus* of King George's Sound?

Or to turn to the higher Vertebrata—in what sense are the Liassic Chelonia inferior to those which now exist? How are the Cretaceous Ichthyosauria, Plesiosauria, or Pterosauria less embyronic, or more differentiated, species than those of the Lias?

Or lastly, in what circumstance is the *Phascolotherium* more embryonic, or of a more generalized type, than the modern Opossum; or a *Lophiodon*, or a *Palæotherium*, than a modern *Tapirus* or *Hyrax*?

These examples might be almost indefinitely multiplied, but surely they are sufficient to prove that the only safe and unquestionable testimony we can procure—positive evidence—fails to demonstrate

any sort of progressive modification toward a less embryonic, or less generalized, type in a great many groups of animals of long-continued geological existence. In these groups there is abundant evidence of variation—none of what is ordinarily understood as progression; and, if the known geological record is to be regarded as even any considerable fragment of the whole, it is inconceivable that any theory of a necessarily progressive development can stand, for the numerous orders and families cited afford no trace of such a process.

But it is a most remarkable fact, that, while the groups which have been mentioned, and many besides, exhibit no sign of progressive modification, there are others, co-existing with them, under the same conditions, in which more or less distinct indications of such a process seem to be traceable. Among such indications I may remind you of the predominance of Holostome Gasteropoda in the older rocks as compared with that of Siphonostome Gasteropoda in the later. A case less open to the objection of negative evidence, however, is that afforded by the Tetrabranchiate Cephalopoda, the forms of the shells and of the septal sutures exhibiting a certain increase of complexity in the newer genera. Here, however, one is met at once with the occurrence of *Orthoceras* and *Baculites* at the two ends of the series, and of the fact that one of the simplest genera, *Nautilus*, is that which now exists.

The Crinoidea, in the abundance of stalked forms in the ancient formations as compared with their present rarity, seem to present us with a fair case of modification from a more embryonic toward a less embryonic condition. But then, on careful consideration of the facts, the objection arises that the stalk, calyx, and arms of the palæozoic Crinoid are exceedingly different from the corresponding organs of a larval *Comatula*; and it might with perfect justice be argued that *Actinocrinus* and *Eucalyptocrinus*, for example, depart to the full as widely, in one direction, from the stalked embryo of *Comatula*, as *Comatula* itself does in the other.

The Echinidea, again, are frequently quoted as exhibiting a gradual passage from a more generalized to a more specialized type, seeing that the elongated, or oval, Spatangoids appear after the spheroidal Echinoids. But here it might be argued, on the other hand, that the spheroidal Echinoids, in reality, depart further from the general plan and from the embryonic form than the elongated Spatangoids do; and that the peculiar dental apparatus and the pedicellariæ of the former are marks of at least as great differentiation as the petaloid ambulacra and semitæ of the latter.

Once more, the prevalence of Macrurous before Brachyurous Podophthalmia is, apparently, a fair piece of evidence in favor of progressive modification in the same order of Crustacea; and yet the case will not stand much sifting, seeing that the Macrurous Podophthalmia depart as far in one direction from the common type of Podophthalmia, or from any embryonic condition of the Brachyura, as the Brachyura do in the other; and that the middle terms between Macrura and Brachyura—the Anomura—are little better represented in the older Mesozoic rocks than the Brachyura are.

None of the cases of progressive modification which are cited from among the Invertebrata appear to me to have a foundation less open to criticism than these; and if this be so, no careful reasoner would, I think, be inclined to lay very great stress upon them. Among the Vertebrata, however, there are a few examples which appear to be far less open to objection.

It is, in fact, true of several groups of Vertebrata which have lived through a considerable range of time, that the endoskeleton (more particularly the spinal column) of the older genera presents a less ossified, and, so far, less differentiated, condition than that of the younger genera. Thus the Devonian Ganoids, though almost all members of the same sub-order as *Polypterus*, and presenting numerous important resemblances to the existing genus, which possesses biconcave vertebræ, are, for the most part, wholly devoid of ossified vertebral centra. The Mesozoic Lepidosteidæ, again, have, at most, biconcave vertebræ, while the existing *Lepidosteus* has Salamandroid, opisthocœlous, vertebræ. So, none of the Palæozoic Sharks have

shown themselves to be possessed of ossified vertebræ, while the majority of modern Sharks possess such vertebræ. Again, the more ancient Crocodilia and Lacertilia have vertebræ with the articular facets of their center flattened or biconcave, while the modern members of the same group have them procœlous. But the most remarkable examples of progressive modification of them vertebral column, in correspondence with geological age, are those afforded by the Pycnodonts among fish, and the Labyrinthodonts among Amphibia.

The late able ichthyologist Heckel pointed out the fact, that, while the Pycnodonts never possess true vertebral centra, they differ in the degree of expansion and extension of the ends of the bony arches of the vertebræ upon the sheath of the notochord; the Carboniferous forms exhibiting hardly any such expansion, while the Mesozoic genera present a greater and greater development, until, in the Tertiary forms, the expanded ends become suturally united so as to form a sort of false vertebra. Hermann von Meyer, again, to whose luminous researches we are indebted for our present large knowledge of the organization of the older Labyrinthodonts, has proved that the Carboniferous *Archegosaurus* had very imperfectly developed vertebral centra, while the Triassic *Mastodonsaurus* had the same parts completely ossified.

The regularity and evenness of the dentition of the *Anoplotherium*, as contrasted with that of existing Artiodactyles, and the assumed nearer approach of the dentition of certain ancient Carnivores to the typical arrangement, have also been cited as exemplifications of a law of progressive development, but I know of no other cases based on positive evidence which are worthy of particular notice.

What then does an impartial survey of the positively ascertained truths of palæontology testify in relation to the common doctrines of progressive modification, which suppose that modification to have taken place by a necessary progress from more to less embryonic forms, or from more to less generalized types, within the limits of the period represented by the fossiliferous rocks?

It negatives those doctrines; for it either shows us no evidence of any such modification, or demonstrates it to have been very slight; and as to the nature of that modification, it yields no evidence whatsoever that the earlier members of any long-continued group were more generalized in structure than the later ones. To a certain extent, indeed, it may be said that imperfect ossification of the vertebral column is an embryonic character; but, on the other hand, it would be extremely incorrect to suppose that the vertebral columns of the older Vertebrata are in any sense embryonic in their whole structure.

Obviously, if the earliest fossiliferous rocks now known are coëval with the commencement of life, and if their contents give us any just conception of the nature and the extent of the earliest fauna and flora, the insignificant amount of modification which can be demonstrated to have taken place in any one group of animals, or plants, is quite incompatible with the hypothesis that all living forms are the results of a necessary process of progressive development, entirely comprised within the time represented by the fossiliferous rocks.

Contrariwise, any admissible hypothesis of progressive modification must be compatible with persistence without progression, through indefinite periods. And should such an hypothesis eventually be proved to be true, in the only way in which it can be demonstrated, viz., by observation and experiment upon the existing forms of life, the conclusion will inevitably present itself, that the Palæozoic, Mesozoic, and Cainozoic faunæ and floræ, taken together, bear somewhat the same proportion to the whole series of living beings which have occupied this globe as the existing fauna and flora do to them.

Such are the results of palæontology as they appear, and have for some years appeared, to the mind of an inquirer who regards that study simply as one of the applications of the great biological sciences, and who desires to see it placed upon the same sound basis as other branches of physical inquiry. If the arguments which have been brought forward are valid, probably no one, in view of the present state of opinion, will be

inclined to think the time wasted which has been spent upon their elaboration.

A LIBERAL EDUCATION, AND WHERE TO FIND IT.

THE business which the South London Working Men's College has undertaken is a great work; indeed, I might say, that Education, with which that college proposes to grapple, is the greatest work of all those which lie ready to a man's hand just at present.

And, at length, this fact is becoming generally recognized. You cannot go anywhere without hearing a buzz of more or less confused and contradictory talk on this subject—nor can you fail to notice that, in one point at any rate, there is a very decided advance upon like discussions in former days. Nobody outside the agricultural interest now dares to say that education is a bad thing. If any representative of the once large and powerful party, which, in former days, proclaimed this opinion, still exists in a semi-fossil state, he keeps his thoughts to himself. In fact, there is a chorus of voices, almost distressing in their harmony, raised in favor of the doctrine that education is the great panacea for human troubles, and that, if the country is not shortly to go to the dogs, everybody must be educated.

The politicians tell us, "you must educate the masses because they are going to be masters." The clergy join in the cry for education, for they affirm that the people are drifting away from church and chapel into the broadest infidelity. The manufacturers and the capitalists swell the chorus lustily. They declare that ignorance makes bad workmen; that England will soon be unable to turn out cotton goods, or steam engines, cheaper than other people; and then, Ichabod! Ichabod! the glory will be departed from us. And a few voices are lifted up in favor of the doctrine that the masses should be educated because they are men and women with unlimited capacities of being, doing, and suffering, and that it is as true now, as ever it was, that the people perish for lack of knowledge.

These members of the minority, with whom I confess I have a good deal of sympathy, are doubtful whether any of the other reasons urged in favor of the education of the people are of much value—whether, indeed, some of them are based upon either wise or noble grounds of action. They question if it be wise to tell people that you will do for them, out of fear of their power, what you have left undone, so long as your only motive was compassion for their weakness and their sorrows. And, if ignorance of everything which it is needful a ruler should know is likely to do so much harm in the governing classes of the future, why is it, they ask reasonably enough, that such ignorance in the governing classes of the past has not been viewed with equal horror?

Compare the average artisan and the average country squire, and it may be doubted if you will find a pin to choose between the two in point of ignorance, class feeling, or prejudice. It is true that the ignorance is of a different sort—that the class feeling is in favor of a different class, and that the prejudice has a distinct favor of wrong-headedness in each case—but it is questionable if the one is either a bit better, or a bit worse, than the other. The old protectionist theory is the doctrine of trades unions as applied by the squires, and the modern trades unionism is the doctrine of the squires applied by the artisans. Why should we be worse off under one *régime* than under the other?

Again, this sceptical minority asks the clergy to think whether it is really want of education which keeps the masses away from their ministrations—whether the most completely educated men are not as open to reproach on this score as the workmen; and whether, perchance, this may not indicate that it is not education which lies at the bottom of the matter?

Once more, these people, whom there is no pleasing, venture to doubt whether the glory which rests upon being able to undersell all the rest of the world, is a very safe kind of glory—whether we may not purchase it too dear; especially if we allow education, which ought to be directed to the making of men, to be diverted into a process of manufacturing human tools, wonderfully adroit in the exercise of some technical industry, but good for nothing else.

And, finally, these people inquire whether it is the masses alone who need a reformed and improved education. They ask whether the richest of our public schools might not well be made to supply knowledge, as well as gentlemanly habits, a strong class feeling, and eminent proficiency in cricket. They seem to think that the noble foundations of our old universities are hardly fulfilling their functions in their present posture of half-clerical seminaries, half racecourses, where men are trained to win a senior wranglership or a double-first, as horses are trained to win a cup, with as little reference to the needs of after-life in the case of the man as in that of the racer. And, while as zealous for education as the rest, they affirm that, if the education of the richer classes were such as to fit them to be the leaders and the governors of the poorer; and if the education of the poorer classes were such as to enable them to appreciate really wise guidance and good governance; the politicians need not fear mob-law, nor the clergy lament their want of flocks, nor the capitalists prognosticate the annihilation of the prosperity of the country.

Such is the diversity of opinion upon the why and the wherefore of education. And my hearers will be prepared to expect that the practical recommendations which are put forward are not less discordant. There is a loud cry for compulsory education. We Eng-

* An Address to the South London Working Men's College, delivered January 4th, 1868.

lish, in spite of constant experience to the contrary, preserve a touching faith in the efficacy of acts of parliament; and I believe we should have compulsory education in the course of next session, if there were the least probability that half a dozen leading statesmen of different parties would agree what that education should be.

Some hold that education without theology is worse than none. Others maintain, quite as strongly, that education with theology is in the same predicament. But this is certain, that those who hold the first opinion can by no means agree what theology should be taught; and that those who maintain the second are in a small minority.

At any rate, "make people learn to read, write, and cipher," say a great many; and the advice is undoubtedly sensible as far as it goes. But, as has happened to me in former days, those who, in despair of getting anything better, advocate this measure, are met with the objection that it is very like making a child practice the use of a knife, fork, and spoon, without giving it a particle of meat. I really don't know what reply is to be made to such an objection.

But it would be unprofitable to spend more time in disentangling, or rather in showing up the knots in, the raveled skeins of our neighbors. Much more to the purpose is it to ask if we possess any clue of our own which may guide us among these entanglements. And by way of a beginning, let us ask ourselves—What is education? Above all things, what is our ideal of a thoroughly liberal education?—of that education which, if we could begin life again, we would give ourselves—of that education which, if we could mould the fates to our own will, we would give our children. Well, I know not what may be your conceptions upon this matter, but I will tell you mine, and I hope I shall find that our views are not very discrepant.

Suppose it were perfectly certain that the life and fortune of everyone of us would, one day or other, depend upon his winning or losing a game at chess. Don't you think that we should all consider it to be a primary duty to learn at least the names and the moves of the pieces; to have a notion of a gambit, and a keen eye for all the means of giving and getting out of check? Do you not think that we should look with a disapprobation amounting to scorn upon the father who allowed his son, or the state which allowed its members, to grow up without knowing a pawn from a knight?

Yet it is a very plain and elementary truth that the life, the fortune, and the happiness of every one of us, and, more or less, of those who are connected with us, do depend upon our knowing something of the rules of a game infinitely more difficult and complicated than chess. It is a game which has been played for untold ages, every man and woman of us being one of the two players in a game of his or her own. The chess-board is the world, the pieces are the phenomena of the universe, the rules of the game are what we call the laws of Nature. The player on the other side is hidden from us. We know that his play is always fair, just, and patient. But also we know, to our cost, that he never overlooks a mistake, or makes the smallest allowance for ignorance. To the man who plays well, the highest stakes are paid, with that sort of overflowing generosity with which the strong shows delight in strength. And one who plays ill is checkmated—without haste, but without remorse. My metaphor will remind some of you of the famous picture in which Retzsch has depicted Satan playing at chess with man for his soul. Substitute for the mocking fiend in that picture, a calm, strong angel who is playing for love, as we say, and would rather lose than win—and I should accept it as an image of human life.

Well, what I mean by Education is learning the rules of this mighty game. In other words, education is the instruction of the intellect in the laws of Nature, under which name I include not merely things and their forces, but men and their ways; and the fashioning of the affections and of the will into an earnest and loving desire to move in harmony with those laws. For me, education means neither more nor less than this. Anything which professes to call itself education must be tried by this standard, and if it fails to stand the test, I will not call it education, whatever may be the force of authority or of numbers upon the other side.

It is important to remember that, in strictness, there is no such thing as an uneducated man. Take an extreme case. Suppose that an adult man, in the full vigor of his faculties, could be suddenly placed in the world, as Adam is said to have been, and then left to do as he best might. How long would he be left uneducated? Not five minutes. Nature would begin to teach him, through the eye, the ear, the touch, the properties of objects. Pain and pleasure would be at his elbow telling him to do this and avoid that; and by slow degrees the man would receive an education, which, if narrow, would be thorough, real, and adequate to his circumstances, though there would be no extras and very few accomplishments.

And if to this solitary man entered a second Adam, or, better still, an Eve, a new and greater world, that of social and moral phenomena, would be revealed. Joys and woes, compared with which all others might seem but faint shadows, would spring from the new relations. Happiness and sorrow would take the place of the coarser monitors, pleasure and pain; but conduct would still be shaped by the observation of the natural consequences of actions; or, in other words, by the laws or the nature of man.

To every one of us the world was once as fresh and new as to Adam. And then, long before we were susceptible of any other mode of instruction, Nature took us in hand, and every minute of waking life

brought its educational influence, shaping our actions into rough accordance with Nature's laws, so that we might not be ended untimely by too gross disobedience. Nor should I speak of this process of education as past, for anyone, be he as old as he may. For every man, the world is as fresh as it was at the first day, and as full of untold novelties for him who has the eyes to see them. And Nature is still continuing her patient education of us in that great university, the universe, of which we are all members—Nature having no Test-Acts.

Those who take honors in Nature's university, who learn the laws which govern men and things and obey them, are the really great and successful men in this world. The great mass of mankind are the "Poll" who pick up just enough to get through without much discredit. Those who won't learn at all are plucked; and then you can't come up again. Nature's pluck means extermination.

Thus the question of compulsory education is settled so far as Nature is concerned. Her bill on that question was framed and passed long ago. But, like all compulsory legislation, that of Nature is harsh and wasteful in its operation. Ignorance is visited as sharply as wilful disobedience—incapacity meets with the same punishment as crime. Nature's discipline is not even a word and a blow, and the blow first; but the blow without the word. It is left to you to find out why your ears are boxed.

The object of what we commonly call education—that education in which man intervenes and which I shall distinguish as artificial education—is to make good these defects in Nature's methods; to prepare the child to receive Nature's education, neither incapably nor ignorantly, nor with wilful disobedience; and to understand the preliminary symptoms of her displeasure, without waiting for the box on the ear. In short, all artificial education ought to be an anticipation of natural education. And a liberal education is an artificial education, which has not only prepared a man to escape the great evils of disobedience to natural laws, but has trained him to appreciate and to seize upon the rewards which Nature scatters with as free a hand as her penalties.

That man, I think, has had a liberal education, who has been so trained in youth that his body is the ready servant of his will, and does with ease and pleasure all the work that, as a mechanism, it is capable of; whose intellect is a clear, cold, logic engine, with all its parts of equal strength, and in smooth working order; ready, like a steam-engine, to be turned to any kind of work, and spin the gossamers as well as forge the anchors of the mind; whose mind is stored with a knowledge of the great and fundamental truths of Nature and of the laws of her operations; one who, no stunted ascetic, is full of life and fire, but whose passions are trained to come to heel by a vigorous will, the servant of a tender conscience; who has learned to love all beauty, whether of Nature or of art, to hate all vileness, and to respect others as himself.

Such an one and no other, I conceive, has had a liberal education; for he is, as completely as a man can be, in harmony with Nature. He will make the best of her, and she of him. They will get on together rarely; she as his ever-beneficent mother; he as her mouth-piece, her conscious self, her minister and interpreter.

Where is such an education as this to be had? Where is there any approximation to it? Has anyone tried to found such an education? Looking over the length and breadth of these islands, I am afraid that all these questions must receive a negative answer. Consider our primary schools, and what is taught in them. A child learns:

1. To read, write, and cipher, more or less well; but in a very large proportion of cases not so well as to take pleasure in reading, or to be able to write the commonest letter properly.

2. A quantity of dogmatic theology, of which the child, nine times out of ten, understands next to nothing.

3. Mixed up with this, so as to seem to stand or fall with it, a few of the broadest and simplest principles of morality. This, to my mind, is much as if a man of science should make the story of the fall of the apple in Newton's garden an integral part of the doctrine of gravitation, and teach it as of equal authority with the law of the inverse squares.

4. A good deal of Jewish history and Syrian geography, and, perhaps, a little something about English history and the geography of the child's own country. But I doubt if there is a primary school in England in which hangs a map of the hundred in which the village lies, so that the children may be practically taught by it what a map means.

5. A certain amount of regularity, attentive obedience, respect for others; obtained by fear, if the master be incompetent or foolish; by love and reverence, if he be wise.

So far as this school course embraces a training in the theory and practice of obedience to the moral laws of Nature, I gladly admit, not only that it contains a valuable educational element, but that, so far, it deals with the most valuable and important part of all education. Yet, contrast what is done in this direction with what might be done; with the time given to matters of comparatively no importance; with the absence of any attention to things of the highest moment; and one is tempted to think of Falstaff's bill and "the halfpenny worth of bread to all that quantity of sack."

Let us consider what a child thus "educated" knows, and what it does not know. Begin with the most important topic of all—morality, as the guide of conduct. The child knows well enough that some acts meet with approbation and some with disapprobation. But it has never heard that there lies

in the nature of things a reason for every moral law, as cogent and as well defined as that which underlies every physical law; that stealing and lying are just as certain to be followed by evil consequences, as putting your hand in the fire, or jumping out of a garret window. Again, though the scholar may have been made acquainted, in dogmatic fashion, with the broad laws of morality, he has had no training in the application of those laws to the difficult problems which result from the complex conditions of modern civilization. Would it not be very hard to expect anyone to solve a problem in conic sections who had merely been taught the axioms and definitions of mathematical science?

A workman has to bear hard labor, and perhaps privation, while he sees others rolling in wealth, and feeding their dogs with what would keep his children from starvation. Would it not be well to have helped that man to calm the natural promptings of discontent by showing him, in his youth, the necessary connection of the moral law which prohibits stealing with the stability of society—by proving to him, once for all, that it is better for his own people, better for himself, better for future generations, that he should starve than steal? If you have no foundation of knowledge, or habit of thought, to work upon, what chance have you of persuading a hungry man that a capitalist is not a thief "with a circumbendibus"? And if he honestly believes that, of what avail is it to quote the commandment against stealing, when he proposes to make the capitalist disgorge?

Again, the child learns absolutely nothing of the history or the political organization of his own country. His general impression is, that everything of much importance happened a very long while ago; and that the queen and the gentlefolks govern the country much after the fashion of King David and the elders and nobles of Israel—his sole models. Will you give a man with this much information a vote? In easy times he sells it for a pot of beer. Why should he not? It is of about as much use to him as a chignon, and he knows as much what to do with it, for any other purpose. In bad times, on the contrary, he applies his simple theory of government, and believes that his rulers are the cause of his sufferings—a belief which sometimes bears remarkable practical fruits.

Least of all, does the child gather from this primary "education" of ours a conception of the laws of the physical world, or of the relations of cause and effect therein. And this is the more to be lamented, as the poor are especially exposed to physical evils, and are more interested in removing them than any other class of the community. If anyone is concerned in knowing the ordinary laws of mechanics, one would think it is the hand-laborer, whose daily toil lies among levers and pulleys; or among the other implements of artisan work. And if anyone is interested in the laws of health, it is the poor workman, whose strength is wasted by ill-prepared food, whose health is sapped by bad ventilation and bad drainage, and half whose children are massacred by disorders which might be prevented. Not only does our present primary education carefully abstain from hinting to the workman that some of his greatest evils are traceable to mere physical agencies, which could be removed by energy, patience, and frugality; but it does worse—it renders him, so far as it can, deaf to those who could help him, and tries to substitute an Oriental submission to what is falsely declared to be the will of God, for his natural tendency to strive after a better condition.

What wonder, then, if very recently an appeal has been made to statistics for the profoundly foolish purpose of showing that education is of no good—that it diminishes neither misery nor crime, among the masses of mankind? I reply, why should the thing which has been called education do either the one or the other? If I am a knave or a fool, teaching me to read and write won't make me less of either one or the other—unless somebody shows me how to put my reading and writing to wise and good purposes.

Suppose any one were to argue that medicine is of no use, because it could be proved statistically that the percentage of deaths was just the same among people who had been taught how to open a medicine-chest, and among those who did not so much as know the key by sight. The argument is absurd; but it is not more preposterous than that against which I am contending. The only medicine for suffering, crime, and all the other woes of mankind, is wisdom. Teach a man to read and write, and you have put into his hands the great keys of the wisdom box. But it is quite another matter whether he ever opens the box or not. And he is as likely to poison as to cure himself, if, without guidance, he swallows the first drug that comes to hand. In these times a man may as well be purblind as unable to read—lame, as unable to write. But I protest that, if I thought the alternative were a necessary one, I would rather that the children of the poor should grow up ignorant of both these mighty arts, than that they should remain ignorant of that knowledge to which these arts are means.

It may be said that all these animadversions may apply to primary schools, but that the higher schools, at any rate, must be allowed to give a liberal education. In fact, they professedly sacrifice everything else to this object.

Let us inquire into this matter. What do the higher schools, those to which the great middle class of the country sends its children, teach, over and above the instruction given in the primary schools? There is a little more reading and writing of English. But, for all that, everyone knows that it is a rare thing to find a boy of the middle or upper classes

who can read aloud decently, or who can put his thoughts on paper in clear and grammatical (to say nothing of good or elegant) language. The "ciphering" of the lower schools expands into elementary mathematics in the higher; into arithmetic, with a little algebra, a little Euclid. But I doubt if one boy in five hundred has ever heard the explanation of a rule of arithmetic, or knows his Euclid otherwise than by rote.

Of theology, the middle class schoolboy gets rather less than poorer children, less absolutely and less relatively, because there are so many other claims upon his attention. I venture to say that, in the great majority of cases, his ideas on this subject when he leaves school are of the most shadowy and vague description, and associated with painful impressions of the weary hours spent in learning collects and catechism by heart.

Modern geography, modern history, modern literature, the English language as a language; the whole circle of the sciences, physical, moral, and social, are even more completely ignored in the higher than in the lower schools. Up till within a few years back, a boy might have passed through anyone of the great public schools with the greatest distinction and credit, and might never so much as have heard of one of the subjects I have just mentioned. He might never have heard that the earth goes round the sun; that England underwent a great revolution in 1688, and France another in 1789; that there once lived certain notable men called Chaucer, Shakespeare, Milton, Voltaire, Goethe, Schiller. The first might be a German and the last an Englishman for anything he could tell you to the contrary. And as for science, the only idea the word would suggest to his mind would be dexterity in boxing.

I have said that this was the state of things a few years back, for the sake of the few righteous who are to be found among the educational cities of the plain. But I would not have you too sanguine about the result, if you sound the minds of the existing generation of public schoolboys, on such topics as those I have mentioned.

Now let us pause to consider this wonderful state of affairs; for the time will come when Englishmen will quote it as the stock example of the stolid stupidity of their ancestors in the nineteenth century. The most thoroughly commercial people, the greatest voluntary wanderers and colonists the world has ever seen, are precisely the middle classes of this country. If there be a people which has been busy making history on the great scale for the last three hundred years—and the most profoundly interesting history—history which, if it happened to be that of Greece or Rome, we should study with avidity—it is the English. If there be a people which, during the same period, has developed a remarkable literature, it is our own. If there be a nation whose prosperity depends absolutely and wholly upon their mastery over the forces of Nature, upon their intelligent apprehension of, and obedience to, the laws of the creation and distribution of wealth, and of the stable equilibrium of the forces of society, it is precisely this nation. And yet this is what these wonderful people tell their sons: "At the cost of from one to two thousand pounds of our hard-earned money, we devote twelve of the most precious years of your lives to school. Then you shall toil, or be supposed to toil; but there you shall not learn one single thing of all those you will most want to know directly you leave school and enter upon the practical business of life. You will in all probability go into business, but you shall not know where, or how, any article of commerce is produced, or the difference between an export or an import, or the meaning of the word 'capital.' You will very likely settle in a colony, but you shall not know whether Tasmania is part of New South Wales, or *vice versâ*.

"Very probably you may become a manufacturer, but you shall not be provided with the means of understanding the working of one of your own steam-engines, or the nature of the raw products you employ; and, when you are asked to buy a patent, you shall not have the slightest means of judging whether the inventor is an impostor who is contravening the elementary principles of science, or a man who will make you as rich as Crœsus.

"You will very likely get into the House of Commons. You will have to take your share in making laws which may prove a blessing or a curse to millions of men. But you shall not hear one word respecting the political organization of your country; the meaning of the controversy between free-traders and protectionists shall never have been mentioned to you; you shall not so much as know that there are such things as economical laws.

"The mental power which will be of most importance in your daily life will be the power of seeing things as they are without regard to authority; and of drawing accurate general conclusions from particular facts. But at school and at college you shall know of no source of truth but authority; nor exercise your reasoning faculty upon anything but deduction from that which is laid down by authority.

"You will have to weary your soul with work, and many a time eat your bread in sorrow and in bitterness, and you shall not have learned to take refuge in the great source of pleasure without alloy, the serene resting-place for worn human nature—the world of art."

Said I not rightly that we are a wonderful people? I am quite prepared to allow, that education entirely devoted to these omitted subjects might not be a completely liberal education. But is an education which ignores them all a liberal education? Nay, is it too much to say that the education which should embrace these subjects and no others, would be a real education, though an incom-

plete one; while an education which omits them is really not an education at all, but a more or less useful course of intellectual gymnastics?

For what does the middle-class school put in the place of all these things which are left out? It substitutes what is usually comprised under the compendious title of the "classics"—that is to say, the languages, the literature, and the history of the ancient Greeks and Romans, and the geography of so much of the world as was known to these two great nations of antiquity. Now, do not expect me to depreciate the earnest and enlightened pursuit of classical learning. I have not the least desire to speak ill of such occupations, nor any sympathy with those who run them down. On the contrary, if my opportunities had lain in that direction, there is no investigation into which I could have thrown myself with greater delight than that of antiquity.

What science can present greater attractions than philology? How can a lover of literary excellence fail to rejoice in the ancient masterpieces? And with what consistency could I, whose business lies so much in the attempt to decipher the past, and to build up intelligible forms out of the scattered fragments of long-extinct beings, fail to take a sympathetic, though an unlearned, interest in the labors of a Niebuhr, a Gibbon, or a Grote? Classical history is a great section of the palæontology of man; and I have the same double respect for it as for other kinds of palæontology—that is to say, a respect for the facts which it establishes as for all facts, and a still greater respect for it as a preparation for the discovery of a law of progress.

But if the classics were taught as they might be taught—if boys and girls were instructed in Greek and Latin, not merely as languages, but as illustrations of philological science; if a vivid picture of life on the shores of the Mediterranean, two thousand years ago, were imprinted on the minds of scholars; if ancient history were taught, not as a weary series of feuds and fights, but traced to its causes in such men placed under such conditions; if, lastly, the study of the classical books were followed in such a manner as to impress boys with their beauties, and with the grand simplicity of their statement of the everlasting problems of human life, instead of with their verbal and grammatical peculiarities; I still think it as little proper that they should form the basis of a liberal education for our contemporaries, as I should think it fitting to make that sort of palæontology with which I am familiar the backbone of modern education.

It is wonderful how close a parallel to classical training could be made out of that palæontology to which I refer. In the first place, I could get up an osteological primer so arid, so pedantic in its terminology, so altogether distasteful to the youthful mind, as to beat the recent famous production of the head-masters out of the field in all these excellences. Next, I could exercise my boys upon easy fossils, and bring out all their powers of memory and all their ingenuity in the application of my osteo-grammatical rules to the interpretation or construing of those fragments. To those who had reached the higher classes, I might supply odd bones to be built up into animals, giving great honor and reward to him who succeeded in fabricating monsters most entirely in accordance with the rules. That would answer to verse-making and essay-writing in the dead languages.

To be sure, if a great comparative anatomist were to look at these fabrications, he might shake his head, or laugh. But what then? Would such a catastrophe destroy the parallel? What think you would Cicero, or Horace, say to the production of the best sixth form going? And would not Terence stop his ears and run out if he could be present at an English performance of his own plays? Would Hamlet, in the mouths of a set of French actors, who should insist on pronouncing English after the fashion of their own tongue, be more hideously ridiculous?

But it will be said that I am forgetting the beauty and the human interest which appertain to classical studies. To this I reply that it is only a very strong man who can appreciate the charms of a landscape, as he is toiling up a steep hill, along a bad road. What with short-windedness, stones, ruts, and a pervading sense of the wisdom of rest and be thankful, most of us have little enough sense of the beautiful under these circumstances. The ordinary schoolboy is precisely in this case. He finds Parnassus uncommonly steep, and there is no chance of his having much time or inclination to look about him till he gets to the top. And nine times out of ten he does not get to the top.

But if this be a fair picture of the results of classical teaching at its best—and I gather from those who have authority to speak on such matters that it is so—what is to be said of classical teaching at its worst, or in other words, of the classics of our ordinary middle class schools?* I will tell you. It means getting up endless forms and rules by heart. It means turning Latin and Greek into English, for the mere sake of being able to do it, and without the smallest regard to the worth or worthlessness of the author read. It means the learning of innumerable, not always decent, fables in such a shape that the meaning they once had is dried up into utter trash; and the only impression left upon a boy's mind is, that the people who believed such things must have been the greatest idiots the world ever saw. And it means, finally, that after a dozen years spent at this

* For a justification of what is here said about these schools, see that valuable book, "Essays on a Liberal Education," *passim*.

kind of work, the sufferer shall be incompetent to interpret a passage in an author he has not already got up; that he shall loathe the sight of a Greek or Latin book; and that he shall never open, or think of, a classical writer again, until, wonderful to relate, he insists upon submitting his sons to the same process.

These be your gods, O Israel! For the sake of this net result (and respectability) the British father denies his children all the knowledge they might turn to account in life, not merely for the achievement of vulgar success, but for guidance in the great crises of human existence. This is the stone he offers to those whom he is bound by the strongest and tenderest ties to feed with bread.

If primary and secondary education are in this unsatisfactory state, what is to be said to the universities? This is an awful subject, and one I almost fear to touch with my unhallowed hands; but I can tell you what those say who have authority to speak.

The Rector of Lincoln College, in his lately published, valuable "Suggestions for Academical Organization with especial reference to Oxford," tells us (p. 127):

"The colleges were, in their origin, endowments, not for the elements of a general liberal education, but for the prolonged study of special and professional faculties by men of riper age. The universities embraced both these objects. The colleges, while they incidentally aided in elementary education, were specially devoted to the highest learning. . . . .

"This was the theory of the middle-age university and the design of collegiate foundations in their origin. Time and circumstances have brought about a total change. The colleges no longer promote the researches of science, or direct professional study. Here and there college walls may shelter an occasional student, but not in larger proportions than may be found in private life. Elementary teaching of youths under twenty is now the only function performed by the university, and almost the only object of college endowments. Colleges were homes for the life-study of the highest and most abstruse parts of knowledge. They have become boarding-schools in which the elements of the learned languages are taught to youths."

If Mr. Pattison's high position, and his obvious love and respect for his university, be insufficient to convince the outside world that language so severe is yet no more than just, the authority of the Commissioners who reported on the University of Oxford in 1850 is open to no challenge. Yet they write:

"It is generally acknowledged that both Oxford and the country at large suffer greatly from the absence of a body of learned men devoting their lives to the cultivation of science, and to the direction of academical education.

"The fact that so few books of profound research emanate from the University of Oxford, materially impairs its character as a set of learning, and consequently its hold on the respect of the nation."

Cambridge can claim no exemption from the reproaches addressed to Oxford. And thus there seems no escape from the admission that what we fondly call our great seats of learning are simply "boarding-schools" for bigger boys; that learned men are not more numerous in them than out of them; that the advancement of knowledge is not the object of fellows of colleges; that, in the philosophic calm and meditative stillness of their greenswarded courts, philosophy does not thrive, and meditation bears few fruits.

It is my great good fortune to reckon among my friends resident members of both universities, who are men of learning and research, zealous cultivators of science, keeping before their minds a noble ideal of a university, and doing their best to make that ideal a reality; and to me, they would necessarily typify the universities, did not the authoritative statements I have quoted compel me to believe that they are exceptional, and not representative men. Indeed, upon calm consideration, several circumstances lead me to think that the Rector of Lincoln College and the Commissioners cannot be far wrong.

I believe there can be no doubt that the foreigner who should wish to become acquainted with the scientific or the literary activity of modern England, would simply lose his time and his pains if he visited our universities with that object.

And, as for works of profound research on any subject, and, above all, in that classical lore for which the universities profess to sacrifice almost everything else, why, a third-rate, poverty-stricken German university turns out more produce of that kind in one year than our vast and wealthy foundations elaborate in ten.

Ask the man who is investigating any question, profoundly and thoroughly—be it historical, philosophical, philological, physical, literary, or theological; who is trying to make himself master of any abstract subject (except, perhaps, political economy and geology, both of which are intensely Anglican sciences) whether he is not compelled to read half a dozen times as many German as English books? And whether, of these English books, more than one in ten is the work of a fellow of a college, or a professor of an English university?

Is this from any lack of power in the English as compared with the German mind? The countrymen of Grote and of Mill, of Faraday, of Robert Brown, of Lyell, and of Darwin, to go no further back than the contemporaries of men of middle age, can afford to smile at such a suggestion. England can show now, as she has been able to show in every generation since civilization spread over the West, individual men who

hold their own against the world, and keep alive the old tradition of her intellectual eminence.

But, in the majority of cases, these men are what they are in virtue of their native intellectual force, and of a strength of character which will not recognize impediments. They are not trained in the courts of the Temple of Science, but storm the walls of that edifice in all sorts of irregular ways, and with much loss of time and power, in order to obtain their legitimate positions.

Our universities not only do not encourage such men; do not offer them positions, in which it should be their highest duty to do, thoroughly, that which they are most capable of doing; but, as far as possible, university training shuts out of the minds of those among them who are subjected to it the prospect that there is anything in the world for which they are specially fitted. Imagine the success of the attempt to still the intellectual hunger of any of the men I have mentioned, by putting before him, as the object of existence, the successful mimicry of the measure of a Greek song, or the roll of Ciceronian prose! Imagine how much success would be likely to attend the attempt to persuade such men that the education which leads to perfection in such elegancies is alone to be called culture; while the facts of history, the process of thought, the conditions of moral and social existence, and the laws of physical nature, are left to be dealt with as they may, by outside barbarians!

It is not thus that the German universities, from being beneath notice a century ago, have become what they are now—the most intensely cultivated and the most productive intellectual corporations the world has ever seen.

The student who repairs to them sees in the list of classes and of professors a fair picture of the world of knowledge. Whatever he needs to know, there is some one ready to teach him, some one competent to discipline him in the way of learning; whatever his special bent, let him but be able and diligent, and in due time he shall find distinction and a career. Among his professors, he sees men whose names are known and revered throughout the civilized world; and their living example infects him with a noble ambition, and a love for the spirit of work.

The Germans dominate the intellectual world by virtue of the same simple secret as that which made Napoleon the master of old Europe. They have declared *la carrière ouverte aux talents*, and every Bursch marches with a professor's gown in his knapsack. Let him become a great scholar, or man of science, and ministers will compete for his services. In Germany, they do not leave the chance of his holding the office he would render illustrious to the tender mercies of a hot canvass and the final wisdom of a mob of country parsons.

In short, in Germany, the universities are exactly what the Rector of Lincoln and the Commissioners tell us the English universities are not; that is to say, corporations "of learned men devoting their lives to the cultivation of science, and the direction of academical education." They are not "boarding-schools for youths," nor clerical seminaries; but institutions for the higher culture of men, in which the theological faculty is of no more importance or prominence than the rest; and which are truly "universities," since they strive to represent and embody the totality of human knowledge, and to find room for all forms of intellectual activity.

May zealous and clear-headed reformers like Mr. Pattison succeed in their noble endeavors to shape our universities toward some such ideal as this, without losing what is valuable and distinctive in their social tone! But until they have succeeded, a liberal education will be no more obtainable in our Oxford and Cambridge Universities than in our public schools.

If I am justified in my conception of the ideal of a liberal education; and if what I have said about the existing educational institutions of the country is also true, it is clear that the two have no sort of relation to one another; that the best of our schools and the most complete of our university trainings give but a narrow, one-sided, and essentially illiberal education—while the worst give what is really next to no education at all. The South London Working Men's College could not copy any of these institutions if it would. I am bold enough to express the conviction that it ought not if it could.

For what is wanted is the reality and not the mere name of a liberal education; and this College must steadily set before itself the ambition to be able to give that education sooner or later. At present, we are but beginning, sharpening our educational tools, as it were, and, except a modicum of physical science, we are not able to offer much more than is to be found in an ordinary school.

Moral and social science—one of the greatest and most fruitful of our future classes, I hope—at present lacks only one thing in our programme, and that is a teacher. A considerable want, no doubt; but it must be recollected that it is much better to want a teacher than to want the desire to learn.

Further, we need what, for want of a better name, I must call Physical Geography. What I mean is that which the Germans call "*Erdkunde*." It is a description of the earth, of its place and relations to other bodies; of its general structure, and of its great features—winds, tides, mountains, plains; of the chief forms of the vegetable and animal worlds, of the varieties of man. It is the peg upon which the greatest quantity of useful and entertaining scientific information can be suspended.

Literature is not upon the College pro-

gramme; but I hope some day to see it there. For literature is the greatest of all sources of refined pleasure, and one of the great uses of a liberal education is to enable us to enjoy that pleasure. There is scope enough for the purposes of liberal education in the study of the rich treasures of our own language alone. All that is needed is direction, and the cultivation of a refined taste by attention to sound criticism. But there is no reason why French and German should not be mastered sufficiently to read what is worth reading in those languages, with pleasure and with profit.

And finally, by and by, we must have History; treated not as a succession of battles and dynasties; not as a series of biographies; not as evidence that Providence has always been on the side of either Whigs or Tories: but as the development of man in times past, and in other conditions than our own.

But, as it is one of the principles of our College to be self-supporting, the public must lead, and we must follow, in these matters. If my hearers take to heart what I have said about liberal education, they will desire these things, and I doubt not we shall be able to supply them. But we must wait till the demand is made.

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Animal Automatism and Other Essays.

BY

THOMAS H. HUXLEY.

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ANIMAL AUTOMATISM,

AND

OTHER ESSAYS,

VIZ.: SCIENCE AND CULTURE; ELEMENTARY INSTRUCTION IN PHYSIOLOGY; THE BORDER TERRITORY BETWEEN ANIMALS AND PLANTS; UNIVERSITIES, ACTUAL AND IDEAL.

BY THOMAS HENRY HUXLEY, LL.D., F.R.S.

I.

ON THE HYPOTHESIS THAT ANIMALS ARE AUTOMATA, AND ITS HISTORY.*

THE first half of the seventeenth century is one of the great epochs of biological science. For though suggestions and indications of the conceptions which took definite shape, at that time, are to be met with in works of earlier date, they are little more than the shadows which coming truth casts forward; men's knowledge was neither extensive enough, nor exact enough, to show them the solid body of fact which threw these shadows.

But, in the seventeenth century, the idea that the physical processes of life are capable of being explained in the same way as other physical phenomena, and, therefore, that the living body is a mechanism, was proved to be true for certain classes of vital actions; and, having thus taken firm root in irrefragable fact, this conception has not only successfully repelled every assault which has been made upon it, but has steadily grown in force and extent of application, until it is now the expressed or implied fundamental proposition of the whole doctrine of scientific Physiology.

If we ask to whom mankind are indebted for this great service, the general voice will name William Harvey. For, by his discovery of the circulation of the blood in the higher animals, by his explanation of the nature of the mechanism by which that circulation is effected, and by his no less remarkable, though less known, investigations of the process of development, Harvey solidly laid the foundations of all those physical explanations of the functions of sustentation and reproduction which modern physiologists have achieved.

But the living body is not only sustained and reproduced: it adjusts itself to external and internal changes; it moves and feels. The attempt to reduce the endless complexities of animal motion and feeling to law and order is, at least, as important a part of the task of the physiologist as the elucidation of what are sometimes called the vegetative processes. Harvey did not make this attempt himself; but the influence of his work upon the man who did make it is patent and unquestionable. This man was René Descartes, who, though by many years Harvey's junior, died before him; and yet, in his short

* An Address delivered at the meeting of the British Association for the Advancement of Science at Belfast, 1874.

span of fifty-four years, took an undisputed place, not only among the chiefs of philosophy, but amongst the greatest and most original of mathematicians; while, in my belief, he is no less certainly entitled to the rank of a great and original physiologist; inasmuch as he did for the physiology of motion and sensation that which Harvey had done for the circulation of the blood, and opened up that road to the mechanical theory of these processes, which has been followed by all his successors.

Descartes was no mere speculator, as some would have us believe: but a man who knew of his own knowledge what was to be known of the facts of anatomy and physiology in his day. He was an unwearied dissector and observer; and it is said, that, on a visitor once asking to see his library, Descartes led him into a room set aside for dissections, and full of specimens under examination. "There," said he, "is my library."

I anticipate a smile of incredulity when I thus champion Descartes's claim to be considered a physiologist of the first rank. I expect to be told that I have read into his works what I find there, and to be asked, Why is it that we are left to discover Descartes's deserts at this time of day, more than two centuries after his death? How is it that Descartes is utterly ignored in some of the latest works which treat expressly of the subject in which he is said to have been so great?

It is much easier to ask such questions than to answer them, especially if one desires to be on good terms with one's contemporaries; but, if I must give an answer, it is this: The growth of physical science is now so prodigiously rapid, that those who are actively engaged in keeping up with the present, have much ado to find time to look at the past, and even grow into the habit of neglecting it. But, natural as this result may be, it is none the less detrimental. The intellect loses, for there is assuredly no more effectual method of clearing up one's own mind on any subject than by talking it over, so to speak, with men of real power and grasp, who have considered it from a totally different point of view. The parallax of time helps us to the true position of a conception, as the parallax of space helps us to that of a star. And the moral nature loses no less. It is well to turn aside from the fretful stir of the present and to dwell with gratitude and respect upon the services of those "mighty men of old who have gone down to the grave with their weapons of war," but who, while they yet lived, won splendid victories over ignorance. It is well, again, to reflect that the fame of Descartes filled all Europe, and his authority overshadowed it for a century; while now, most of those who know his name think of him, either as a person who had some preposterous notions about vortices and was deservedly annihilated by the great Sir Isaac Newton; or as the apostle of an essentially vicious method of deductive speculation; and that, nevertheless, neither the chatter of shifting opinion, nor the silence of personal oblivion, has in the slightest degree affected the growth of the great ideas of which he was the instrument and the mouthpiece.

It is a matter of fact that the greatest physiologist of the eighteenth century, Haller, in treating of the functions of nerve, does little more than reproduce and enlarge upon the ideas of Descartes. It is a matter of fact that David Hartley, in his remarkable work the "Essay on Man," expressly, though still insufficiently, acknowledges the resemblance of his fundamental conceptions to those of Descartes; and I shall now endeavor to show that a series of propositions, which constitute the foundation and essence of the modern physiology of the nervous system, are fully expressed and illustrated in the works of Descartes.

I. *The brain is the organ of sensation, thought, and emotion; that is to say, some change in the condition of the matter of this organ is the invariable antecedent of the state of consciousness to which each of these terms is applied.*

In the "Principes de la Philosophie" (§ 169), Descartes says:—*

"Although the soul is united to the whole body, its principal functions are, nevertheless, performed in the brain; it is here that it not only understands and imagines, but also feels; and this is effected by the intermediation of the nerves, which extend in the form of delicate threads from the brain to all parts of the body, to which they are attached in such a manner, that we can hardly touch any part of the body without setting the extremity of some nerve in motion. This motion passes along the nerve to that part of the brain which is the common sensorium, as I have sufficiently explained in my Treatise on Dioptrics: and the movements which thus

* I quote, here and always, Cousin's edition of the works of Descartes, as most convenient for reference. It is entitled "Œuvres complètes de Descartes," publiées par Victor Cousin. 1824.

travel along the nerves, as far as that part of the brain with which the soul is closely joined and united, cause it, by reason of their diverse characters, to have different thoughts. And it is these different thoughts of the soul, which arise immediately from the movements that are excited by the nerves in the brain, which we properly term our feelings, or the perceptions of our senses."

Elsewhere,* Descartes, in arguing that the seat of the passions is not (as many suppose) the heart, but the brain, uses the following remarkable language:—

"The opinion of those who think that the soul receives its passions in the heart, is of no weight, for it is based upon the fact that the passions cause a change to be felt in that organ; and it is easy to see that this change is felt, as if it were in the heart, only by the intermediation of a little nerve which descends from the brain to it; just as pain is felt, as if it were in the foot, by the intermediation of the nerves of the foot; and the stars are perceived, as if they were in the heavens, by the intermediation of their light and of the optic nerves. So that it is no more necessary for the soul to exert its functions immediately in the heart, to feel its passions there, than it is necessary that it should be in the heavens to see the stars there."

This definite allocation of all the phenomena of consciousness to the brain as their organ, was a step the value of which it is difficult for us to appraise, so completely has Descartes's view incorporated itself with every-day thought and common language. A lunatic is said to be "crack-brained" or "touched in the head," a confused thinker is "muddle-headed," while a clever man is said to have "plenty of brains;" but it must be remembered that at the end of the last century a considerable, though much over-estimated, anatomist, Bichat, so far from having reached the level of Descartes, could gravely argue that the apparatuses of organic life are the sole seat of the passions, which in no way affect the brain, except so far as it is the agent by which the influence of the passions is transmitted to the muscles.†

Modern physiology, aided by pathology, easily demonstrates that the brain is the seat of all forms of consciousness, and fully bears out Descartes's explanation of the reference of those sensations in the viscera which accompany intense emotion, to these organs. It proves, directly, that those states of consciousness which we call sensations are the immediate consequent of a change in the brain excited by the sensory nerves; and, on the well-known effects of injuries, of stimulants, and of narcotics, it bases the conclusion that thought and emotion are, in like manner, the consequents of physical antecedents.

II. *The movements of animals are due to the change of form of muscles, which shorten and become thicker; and this change of form in a muscle arises from a motion of the substance contained within the nerves which go to the muscle.*

In the "Passions de l'Ame," Art. vii., Descartes writes:—

"Moreover, we know that all the movements of the limbs depend on the muscles, and that these muscles are opposed to one another in such a manner, that when one of them shortens, it draws along the part of the body to which it is attached, and so gives rise to a simultaneous elongation of the muscle which is opposed to it. Then, if it happens, afterward, that the latter shortens, it causes the former to elongate, and draws toward itself the part to which it is attached. Lastly, we know that all these movements of the muscles, as all the senses, depend on the nerves, which are like little threads or tubes, which all come from the brain, and, like it, contain a certain very subtle air or wind, termed the animal spirits."

The property of muscle mentioned by Descartes now goes by the general name of contractility, but his definition of it remains untouched. The long-continued controversy whether contractile substance, speaking generally, has an inherent power of contraction, or whether it contracts only in virtue of an influence exerted by nerve, is now settled in Haller's favor; but Descartes's statement of the dependence of muscular contraction on nerve holds good for the higher forms of muscle, under normal circumstances; so that, although the structure of the various modifications of contractile matter has been worked out with astonishing minuteness—although the delicate physical and chemical changes which accompany muscular contraction have been determined to an extent of which Descartes could not have dreamed, and have quite upset his hypothesis that the cause of the shortening and thickening of the muscle is the flow of animal spirits into it from the nerves—the important and fundamental part of his statement remains perfectly true.

The like may be affirmed of what he says about nerve. We know now that

* "Les Passions de l'Ame," Article xxxiii.
† "Recherches physiologiques sur la Vie et la Mort." Par Xav. Bichat. Art. Sixième.

nerves are not exactly tubes, and that "animal spirits" are myths; but the exquisitely refined methods of investigation of Dubois-Reymond and of Helmholz have no less clearly proved that the antecedent of ordinary muscular contraction is a motion of the molecules of the nerve going to the muscle; and that this motion is propagated with a measurable, and by no means great, velocity, through the substance of the nerve toward the muscle.

With the progress of research, the term "animal spirits" gave way to "nervous fluid," and "nervous fluid" has now given way to "molecular motion of nerve-substance." Our conceptions of what takes place in nerve have altered in the same way as our conceptions of what takes place in a conducting wire have altered, since electricity was shown to be not a fluid, but a mode of molecular motion. The change is of vast importance, but it does not affect Descartes's fundamental idea, that a change in the substance of a motor nerve propagated toward a muscle is the ordinary cause of muscular contraction.

III. *The sensations of animals are due to a motion of the substance of the nerves which connect the sensory organs with the brain.*

In *La Dioptrique* (Discours Quatrième), Descartes explains, more fully than in the passage cited above, his hypothesis of the mode of action of sensory nerves:—

"It is the little threads of which the inner substance of the nerves is composed which subserve sensation. You must conceive that these little threads, being inclosed in tubes, which are always distended and kept open by the animal spirits which they contain, neither press upon nor interfere with one another, and are extended from the brain to the extremities of all the members which are sensitive—in such a manner that the slightest touch which excites the part of one of the members to which a thread is attached, gives rise to a motion of the part of the brain whence it arises, just as by pulling one of the ends of a stretched cord, the other end is instantaneously moved. . . . And we must take care not to imagine that, in order to feel, the soul needs to behold certain images sent by the objects of sense to the brain, as our philosophers commonly suppose; or, at least, we must conceive these images to be something quite different from what they suppose them to be. For as all they suppose is that these images ought to resemble the objects which they represent, it is impossible for them to show how they can be formed by the objects received by the organs of the external senses and transmitted to the brain. And they have had no reason for supposing the existence of these images except this; seeing that the mind is readily excited by a picture to conceive the object which is depicted, they have thought that it must be excited in the same way to conceive those objects which affect our senses by little pictures of them formed in the head; instead of which we ought to recollect that there are many things besides images which may excite the mind, as, for example, signs and words, which have not the least resemblance to the objects which they signify."*

Modern physiology amends Descartes's conception of the mode of action of sensory nerves in detail, by showing that their structure is the same as that of motor nerves; and that the changes which take place in them, when the sensory organs with which they are connected are excited, are of just the same nature as those which occur in motor nerves, when the muscles to which they are distributed are made to contract: there is a molecular change which, in the case of the sensory nerve, is propagated toward the brain. But the great fact insisted upon by Descartes, that no likeness of external things is, or can be, transmitted to the mind by the sensory organs; but that, between the external cause of a sensation and the sensation, there is interposed a mode of motion of nervous matter, of which the state of consciousness is no likeness, but a mere symbol, is of the profoundest importance. It is the physiological foundation of the doctrine of the relativity of knowledge, and a more or less complete idealism is a necessary consequence of it.

For of two alternatives one must be true. Either consciousness is the function of a something distinct from the brain, which we call the soul, and a sensation is the mode in which this soul is affected by the motion of a part of the brain; or there is no soul, and a sensation is something generated by the mode of motion of a part of the brain. In the former case, the phenomena of the senses are purely spiritual affections; in the latter, they are something manufactured by the mechanism of the body, and as unlike the causes which set that mechanism in motion, as the sound of a repeater is unlike the pushing of the spring which gives rise to it.

* Locke ("Human Understanding," Book II., chap. viii. 37) uses Descartes's illustration for the same purpose and warns us that "most of the ideas of sensation are no more the likeness of something existing without us than the names that stand for them are the likeness of our ideas, which yet, upon hearing, they are apt to excite in us," a declaration which paved the way for Berkeley.

The nervous system stands between consciousness and the assumed external world, as an interpreter who can talk with his fingers stands between a hidden speaker and a man who is stone deaf—and Realism is equivalent to a belief on the part of the deaf man, that the speaker must also be talking with his fingers. "Les extrêmes se touchent;" the shibboleth of materialists that "thought is a secretion of the brain," is the Fichtean doctrine that "the phenomenal universe is the creation of the Ego," expressed in other language.

IV. *The motion of the matter of a sensory nerve may be transmitted through the brain to motor nerves, and thereby give rise to contraction of the muscles to which these motor nerves are distributed; and this reflection of motion from a sensory into a motor nerve may take place without volition, or even contrary to it.*

In stating these important truths, Descartes defined that which we now term "reflex action." Indeed he almost uses the term itself, as he talks of the "animal spirits" as "réfléchis,"* from the sensory into the motor nerves. And that this use of the word "reflected" was no mere accident, but that the importance and appropriateness of the idea it suggests was fully understood by Descartes's contemporaries, is apparent from a passage in Willis's well-known essay, "De Animâ Brutorum," published in 1672, in which, in giving an account of Descartes's views, he speaks of the animal spirits being diverted into motor channels, "velut undulatione reflexâ."†

Nothing can be clearer in statement, or in illustration, than the view of reflex action which Descartes gives in the "Passions de l'Ame," Art. xiii.

After recapitulating the manner in which sensory impressions transmitted by the sensory nerves to the brain give rise to sensation, he proceeds :—

"And in addition to the different feelings excited in the soul by these different motions of the brain, the animal spirits, without the intervention of the soul, may take their course toward certain muscles, rather than toward others, and thus move the limbs, as I shall prove by an example. If some one moves his hand rapidly toward our eyes, as if he were going to strike us, although we know that he is a friend, that he does it only in jest, and that he will be very careful to do us no harm, nevertheless it will be hard to keep from winking. And this shows, that it is not by the agency of the soul that the eyes shut, since this action is contrary to that volition which is the only, or at least the chief, function of the soul; but it is because the mechanism of our body is so disposed, that the motion of the hand toward our eyes excites another movement in our brain, and this sends the animal spirits into those muscles which cause the eyelids to close."

Since Descartes's time, experiment has eminently enlarged our knowledge of the details of reflex action. The discovery of Bell has enabled us to follow the tracks of the sensory and motor impulses, along distinct bundles of nerve fibers; and the spinal cord, apart from the brain, has been proved to be a great center of reflex action; but the fundamental conception remains as Descartes left it, and it is one of the pillars of nerve physiology at the present day.

V. *The motion of any given portion of the matter of the brain excited by the motion of a sensory nerve, leaves behind a readiness to be moved in the same way, in that part. Anything which resuscitates the motion gives rise to the appropriate feeling. This is the physical mechanism of memory.*

Descartes imagined that the pineal body (a curious appendage to the upper side of the brain, the function of which, if it have any, is wholly unknown) was the instrument through which the soul received impressions from, and communicated them to, the brain. And he thus endeavors to explain what happens when one tries to recollect something :—

"Thus when the soul wills to remember anything, this volition, causing the [pineal] gland to incline itself in different directions, drives the [animal] spirits toward different regions of the brain, until they reach that part in which are the traces, which the object which it desires to remember has left. These traces are produced thus: those pores of the brain through which the [animal] spirits have previously been driven, by reason of the presence of the object, have thereby acquired a tendency to be opened by the animal spirits which return toward them, more easily than other pores, so that the animal spirits, im-

* "Passions de l'Ame," Art. xxxvi.

† "Quamcumque bruti actionem, velut automati mechanici motum artificialem, in eo consistere quod se primò sensibile aliquod spiritus animales afficiens, eosque introrsum convertens, *sensionem* excitat, à qua mox iidem spiritus, velut undulatione reflexâ denuo retrorsum commoti atque pro concinno ipsius fabricæ organorum, et partium ordine, in certos nervos musculosque determinati, respectivos *membrorum motus* perficiunt,"—WILLIS: "De Animâ Brutorum," p. 5, ed. 1763.

pinging on these pores, enter them more readily than others. By this means they excite a particular movement in the pineal gland, which represents the object to the soul, and causes it to know what it is which it desired to recollect." *

That memory is dependent upon some condition of the brain is a fact established by many considerations—among the most important of which are the remarkable phenomena of aphasia. And that the condition of the brain on which memory depends, is largely determined by the repeated occurrence of that condition of its molecules, which gives rise to the idea of the thing remembered, is no less certain. Every boy who learns his lesson by repeating it exemplifies the fact. Descartes, as we have seen, supposes that the pores of a given part of the brain are stretched by the animal spirits, on the occurrence of a sensation, and that the part of the brain thus stretched, being imperfectly elastic, does not return to exactly its previous condition, but remains more distensible than it was before. Hartley suppose that the vibrations, excited by a sensory, or other, impression, do not die away, but are represented by smaller vibrations or "vibratiuncules," the permanency and intensity of which are in relation with the frequency of repetition of the primary vibrations. Haller has substantially the same idea, but contents himself with the general term "mutationes," to express the cerebral change which is the cause of a state of consciousness. These "mutationes" persist for a long time after the cause which gives rise to them has ceased to operate, and are arranged in the brain according to the order of co-existence and succession of their causes. And he gives these persistent "mutationes" the picturesque name of *vestigia rerum*, "quæ non in mente sed in ipso corpore et in medulla quidem cerebri ineffabili modo incredibiliter minutis notis et copia infinita, inscriptæ sunt." † I do not know that any modern theory of the physical conditions of memory differs essentially from these, which are all children—*mutatis mutandis*—of the Cartesian doctrine. Physiology is, at present, incompetent to say anything positively about the matter, or to go farther than the expression of the high probability, that every molecular change which gives rise to a state of consciousness, leaves a more or less persistent structural modification, through which the same molecular change may be regenerated by other agencies than the cause which first produced it.

Thus far, the propositions respecting the physiology of the nervous system which are stated by Descartes have simply been more clearly defined, more fully illustrated, and, for the most part, demonstrated, by modern physiological research. But there remains a doctrine to which Descartes attached great weight, so that full acceptance of it became a sort of note of a thorough-going Cartesian, but which, nevertheless, is so opposed to ordinary prepossessions that it attained more general notoriety, and gave rise to more discussion, than almost any other Cartesian hypothesis. It is the doctrine, that brute animals are mere machines or automata, devoid not only of reason, but of any kind of consciousness, which is stated briefly in the "Discours de la Méthode," and more fully in the "Réponses aux Quatrièmes Objections," and in the correspondence with Henry More.*

The process of reasoning by which Descartes arrived at this startling conclusion is well shown in the following passage of the "Réponses:"—

"But as regards the souls of beasts, although this is not the place for considering them, and though, without a general exposition of physics, I can say no more on this subject than I have already said in the fifth part of my Treatise on Method; yet, I will further state, here, that it appears to me to be a very remarkable circumstance that no movement can take place, either in the bodies of beasts, or even in our own, if these bodies have not in themselves all the organs and instruments by means of which the very same movements would be accomplished in a machine. So that, even in us, the spirit, or the soul, does not directly move the limbs, but only determines the course of that very subtle liquid which is called the animal spirits, which, running continually from the heart by the brain into the muscles, is the cause of all the movements of our limbs, and often may cause many different motions, one as easily as the other.

"And it does not even always exert this determination; for among the movements which take place in us, there are many which do not depend on the mind at all, such as the beating of the heart, the digestion of food, the nutrition, the respiration, of those who sleep; and, even in those who are awake, walking, singing, and other similar actions, when they are performed without the mind

* "Les Passions de l'Ame," xlii.

† Haller, "Primæ Lineæ," ed. iii. "Sensus Interni," dlviii.

* "Réponse de M. Descartes à M. Morus." [illegible] "Œuvres," tome x. p. 204. "Mais le plus grand de tous les préjugés que nous ayons retenus de notre enfance, est celui de croire que les bêtes pensent," etc.

thinking about them. And, when one who falls from a height throws his hands forwards to save his head, it is in virtue of no ratiocination that he performs this action; it does not depend upon his mind, but takes place merely because his senses being affected by the present danger, some change arises in his brain which determines the animal spirits to pass thence into the nerves, in such a manner as is required to produce this motion, in the same way as in a machine, and without the mind being able to hinder it. Now since we observe this in ourselves, why should we be so much astonished if the light reflected from the body of a wolf into the eye of a sheep has the same force to excite in it the motion of flight.

"After having observed this, if we wish to learn by reasoning, whether certain movements of beasts are comparable to those which are effected in us by the operation of the mind, or, on the contrary, to those which depend only on the animal spirits and the disposition of the organs, it is necessary to consider the difference between the two, which I have explained in the fifth part of the Discourse on Method (for I do not think that any others are discoverable), and then it will easily be seen, that all the actions of beasts are similar only to those which we perform without the help of our minds. For which reason we shall be forced to conclude, that we know of the existence in them of no other principle of motion than the disposition of their organs and the continual affluence of animal spirits produced by the heat of the heart, which attenuates and subtilizes the blood; and, at the same time, we shall acknowledge that we have had no reason for assuming any other principle, except that, not having distinguished these two principles of motion, and seeing that the one, which depends only on the animal spirits and the organs, exists in beasts as well as in us, we have hastily concluded that the other, which depends on mind and on thought, was also possessed by them."

Descartes's line of argument is perfectly clear. He starts from reflex action in man, from the unquestionable fact that, in ourselves, co-ordinate, purposive, actions may take place, without the intervention of consciousness or volition, or even contrary to the latter. As actions of a certain degree of complexity are brought about by mere mechanism, why may not actions of still greater complexity be the result of a more refined mechanism? What proof is there that brutes are other than a superior race of marionettes, which eat without pleasure, cry without pain, desire nothing, know nothing, and only simulate intelligence as a bee simulates a mathematician?*

The Port Royalists adopted the hypothesis that brutes are machines, and are said to have carried its practical applications so far, as to treat domestic animals with neglect, if not with actual cruelty. As late as the middle of the eighteenth century, the problem was discussed very fully and ably by Bouillier, in his "Essai philosophique sur l'Ame des Bêtes," while Condillac deals with it in his "Traité des Animaux;" but since then it has received little attention. Nevertheless, modern research has brought to light a great multitude of facts, which not only show that Descartes's view is defensible, but render it far more defensible than it was in his day.

It must be premised, that it is wholly impossible absolutely to prove the presence or absence of consciousness in anything but one's own brain, though, by analogy, we are justified in assuming its existence in other men. Now if, by some accident, a man's spinal cord is divided, his limbs are paralyzed, so far as his volition is concerned, below the point of injury; and he is incapable of experiencing all those states of consciousness, which, in his uninjured state, would be excited by irritation of those nerves which come off below the injury. If the spinal cord is divided in the middle of the back, for example, the skin of the feet may be cut, or pinched, or burned, or wetted with vitriol, without any sensation of touch, or of pain, arising in consciousness. So far as the man is concerned, therefore, the part of the central nervous system which lies beyond the injury is cut off from consciousness. It must indeed be admitted, that, if any one think fit to maintain that the spinal cord below the injury is conscious, but that it is cut off from any means of making its consciousness known to the other consciousness in the brain, there is no means of driving him from his position by logic. But assuredly there is no way of proving it, and in the matter of consciousness, if in anything, we may hold by the rule, "De non apparentibus et de non existentibus eadem est ratio." However near the brain the

* Malebranche states the view taken by orthodox Cartesians in 1689 very forcibly: "Ainsi dans les chiens, les chats, et les autres animaux, il n'y a ny intelligence, ny âme spirituelle comme on l'entend ordinairement. Ils mangent sans plaisir; ils crient sans douleur; ils croissent sans le sçavoir; ils ne désirent rien; ils ne connoissent rien; et s'ils agissent avec adresse et d'une manière qui marque l'intelligence, c'est que Dieu les faisant pour les conserver, il a conformé leurs corps de telle manière, qu'ils évitent organiquement, sans le sçavoir, tout ce qui peut les détruire et qu'ils semblent craindre." ("Feuillet de Conches. Méditations Métaphysiques et Correspondance de N. Malebranche. Neuvième Méditation." 1841.)

spinal cord is injured, consciousness remains intact, except that the irritation of parts below the injury is no longer represented by sensation. On the other hand, pressure upon the anterior division of the brain, or extensive injuries to it, abolish consciousness. Hence, it is a highly probable conclusion, that consciousness in man depends upon the integrity of the anterior division of the brain, while the middle and hinder divisions of the brain, and the rest of the nervous centers, have nothing to do with it. And it is further highly probable, that what is true for man is true for other vertebrated animals.

We may assume, then, that in a living vertebrated animal, any segment of the cerebro-spinal axis (or spinal cord and brain) separated from that anterior division of the brain which is the organ of consciousness, is as completely incapable of giving rise to consciousness, as we know it to be incapable of carrying out volitions. Nevertheless, this separated segment of the spinal cord is not passive and inert. On the contrary, it is the seat of extremely remarkable powers. In our imaginary case of injury, the man would, as we have seen, be devoid of sensation in his legs, and would have not the least power of moving them. But, if the soles of his feet were tickled, the legs would be drawn up, just as vigorously as they would have been before the injury. We know exactly what happens when the soles of the feet are tickled; a molecular change takes place in the sensory nerves of the skin, and is propagated along them and through the posterior roots of the spinal nerves, which are constituted by them, to the gray matter of the spinal cord. Through that gray matter, the molecular motion is reflected into the anterior roots of the same nerves, constituted by the filaments which supply the muscles of the legs, and, traveling along these motor filaments, reaches the muscles, which at once contract, and cause the limbs to be drawn up.

In order to move the legs in this way, a definite co-ordination of muscular contractions is necessary; the muscles must contract in a certain order and with duly proportioned force; and moreover, as the feet are drawn away from the source of irritation, it may be said that the action has a final cause, or is purposive.

Thus it follows, that the gray matter of the segment of the man's spinal cord, though it is devoid of consciousness, nevertheless responds to a simple stimulus by giving rise to a complex set of muscular contractions, co-ordinated toward a definite end, and serving an obvious purpose.

If the spinal cord of a frog is cut across, so as to provide us with a segment separated from the brain, we shall have a subject parallel to the injured man, on which experiments can be made without remorse; as we have a right to conclude that a frog's spinal cord is not likely to be conscious when a man's is not.

Now the frog behaves just as the man did. The legs are utterly paralyzed, so far as voluntary movement is concerned; but they are vigorously drawn up to the body when any irritant is applied to the foot. But let us study our frog a little farther. Touch the skin of the side of the body with a little acetic acid, which gives rise to all the signs of great pain in an uninjured frog. In this case, there can be no pain, because the application is made to a part of the skin supplied with nerves which come off from the cord below the point of section; nevertheless, the frog lifts up the limb of the same side, and applies the foot to rub off the acetic acid; and, what is still more remarkable, if the limb be held so that the frog cannot use it, it will, by and by, move the limb of the other side, turn it across the body, and use it for the same rubbing process. It is impossible that the frog, if it were in its entirety and could reason, should perform actions more purposive than these: and yet we have most complete assurance that, in this case, the frog is not acting from purpose, has no consciousness, and is a mere insensible machine.

But now suppose that, instead of making a section of the cord in the middle of the body, it had been made in such a manner as to separate the hindermost division of the brain from the rest of the organ, and suppose the foremost two-thirds of the brain entirely taken away. The frog is then absolutely devoid of any spontaneity; it sits upright in the attitude which a frog habitually assumes; and it will not stir unless it is touched; but it differs from the frog which I have just described in this, that, if it be thrown into the water, it begins to swim, and swims just as well as the perfect frog does. But swimming requires the combination and successive co-ordination of a great number of muscular actions. And we are forced to conclude, that the impression made upon the sensory nerves of the skin of the frog by the contact with the water into which it is thrown, causes the

transmission to the central nervous apparatus of an impulse, which sets going a certain machinery by which all the muscles of swimming are brought into play in due co-ordination. If the frog be stimulated by some irritating body, it jumps or walks as well as the complete frog can do. The simple sensory impression, acting through the machinery of the cord, gives rise to these complex combined movements.

It is possible to go a step farther. Suppose that only the anterior division of the brain—so much of it as lies in front of the "optic lobes"—is removed. If that operation is performed quickly and skillfully, the frog may be kept in a state of full bodily vigor for months, or it may be for years; but it will sit unmoved. It sees nothing; it hears nothing. It will starve sooner than feed itself, although food put into its mouth is swallowed. On irritation, it jumps or walks; if thrown into the water it swims. If it be put on the hand, it sits there, crouched, perfectly quiet, and would sit there forever. If the hand be inclined very gently and slowly, so that the frog would naturally tend to slip off, the creature's fore paws are shifted on to the edge of the hand, until he can just prevent himself from falling. If the turning of the hand be slowly continued, he mounts up with great care and deliberation, putting first one leg forward and then another, until he balances himself with perfect precision upon the edge; and, if the turning of the hand is continued, over he goes through the needful set of muscular operations, until he comes to be seated in security, upon the back of the hand. The doing of all this requires a delicacy of co-ordination, and a precision of adjustment of the muscular apparatus of the body, which are only comparable to those of a rope-dancer. To the ordinary influences of light, the frog, deprived of its central hemispheres, appears to be blind. Nevertheless, if the animal be put upon a table, with a book at some little distance between it and the light, and the skin of the hinder part of its body is then irritated, it will jump forward, avoiding the book by passing to the right or left of it. Although the frog, therefore, appears to have no sensation of light, visible objects act through its brain upon the motor mechanism of its body.*

It is obvious, that had Descartes been acquainted with these remarkable results of modern research, they would have furnished him with far more powerful arguments than he possessed in favor of his view of the automatism of brutes. The habits of a frog, leading its natural life, involve such simple adaptations to surrounding conditions, that the machinery which is competent to do so much without the intervention of consciousness, might well do all. And this argument is vastly strengthened by what has been learned in recent times of the marvelously complex operations which are performed mechanically, and to all appearance without consciousness, by men, when, in consequence of injury or disease, they are reduced to a condition more or less comparable to that of a frog, in which the anterior part of the brain has been removed. A case has recently been published by an eminent French physician, Dr. Mesnet, which illustrates this condition so remarkably, that I make no apology for dwelling upon it at considerable length.*

A sergeant of the French army, F——, twenty-seven years of age, was wounded during the battle of Bazeilles, by a ball which fractured his left parietal bone. He ran his bayonet through the Prussian soldier who wounded him, but almost immediately his right arm became paralyzed; after walking about two hundred yards, his right leg became similarly affected, and he lost his senses. When he recovered them, three weeks afterward, in hospital at Mayence, the right half of the body was completely paralyzed, and remained in that condition for a year. At present, the only trace of the paralysis which remains is a slight weakness of the right half of the body. Three or four months after the wound was inflicted, periodical disturbances of the functions of the brain made their appearance, and have continued ever since. The disturbances last from fifteen to thirty hours; the intervals at which they occur being from fifteen to thirty days.

For four years, therefore, the life of this man has been divided into alternating phases—short abnormal states intervening between long normal states.

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* See the remarkable essay of Göltz, "Beiträge zur Lehre von den Functionen der Nervencentren des Frosches," published in 1869. I have repeated Göltz's experiments, and obtained the same results.

* "De l'Automatisme de la Mémoire et du Souvenir, dans le Somnambulisme pathologique." Par le Dr. E. Mesnet, Médecin de l'Hôpital Saint-Antoine. *L'Union Médicale*, Juillet 21 et 23, 1874. My attention was first called to a summary of this remarkable case, which appeared in the *Journal des Débats* for the 7th of August, 1874, by my friend General Strachey, F.R.S.

In the periods of normal life, the ex-sergeant's health is perfect; he is intelligent and kindly, and performs, satisfactorily, the duties of a hospital attendant. The commencement of the abnormal state is ushered in by uneasiness and a sense of weight about the forehead, which the patient compares to the constriction of a circle of iron; and, after its termination, he complains, for some hours, of dullness and heaviness of the head. But the transition from the normal to the abnormal state takes place in a few minutes, without convulsions or cries, and without anything to indicate the change to a bystander. His movements remain free and his expression calm, except for a contraction of the brow, an incessant movement of the eyeballs, and a chewing motion of the jaws. The eyes are wide open, and their pupils dilated. If the man happens to be in a place to which he is accustomed, he walks about as usual; but if he is in a new place, or if obstacles are intentionally placed in his way, he stumbles gently against them, stops, and then, feeling over the objects with his hands, passes on one side of them. He offers no resistance to any change of direction which may be impressed upon him, or to the forcible acceleration or retardation of his movements. He eats, drinks, smokes, walks about, dresses and undresses himself, rises and goes to bed at the accustomed hours. Nevertheless, pins may be run into his body, or strong electric shocks sent through it, without causing the least indication of pain; no odorous substance, pleasant or unpleasant, makes the least impression; he eats and drinks with avidity whatever is offered, and takes asafœtida, or vinegar, or quinine, as readily as water; no noise affects him; and light influences him only under certain conditions. Dr. Mesnet remarks, that the sense of touch alone seems to persist, and indeed to be more acute and delicate than in the normal state; and it is by means of the nerves of touch, almost exclusively, that his organism is brought into relation with the external world. Here a difficulty arises. It is clear, from the facts detailed, that the nervous apparatus by which, in the normal state, sensations of touch are excited, is that by which external influences determine the movements of the body, in the abnormal state. But does the state of consciousness, which we term a tactile sensation, accompany the operation of this nervous apparatus in the abnormal state? or is consciousness utterly absent, the man being reduced to an insensible mechanism?

It is impossible to obtain direct evidence in favor of the one conclusion or the other; all that can be said is, that the case of the frog shows that man may be devoid of any kind of consciousness.

A further difficult problem is this. The man is insensible to sensory impressions made through the ear, the nose, the tongue, and, to a great extent, the eye; nor is he susceptible of pain from causes operating during his abnormal state. Nevertheless, it is possible so to act upon his tactile apparatus, as to give rise to those molecular changes in his sensorium, which are ordinarily the causes of associated trains of ideas. I give a striking example of this process in Dr. Mesnet's words:—

"Il se promenait dans le jardin, sous un massif d'arbres, on lui remet à la main sa canne qu'il avait laissé tomber quelques minutes avant. Il la palpe, promène à plusieurs reprises la main sur la poignée coudée de sa canne—devient attentif—semble prêter l'oreille—et, tout-à-coup, appelle 'Henri!' Puis, 'Les voilà! Ils sont au moins une vingtaine! à nous deux, nous en viendrons à bout!' Et alors portant la main derrière son dos comme pour prendre une cartouche, il fait le mouvement de charger son arme, se couche dans l'herbe à plat ventre, la tête cachée par un arbre, dans la position d'un tirailleur, et suit, l'arme épaulée, tous les mouvements de l'ennemi qu'il croit voir à courte distance."

In a subsequent abnormal period, Dr. Mesnet caused the patient to repeat this scene by placing him in the same conditions. Now, in this case, the question arises whether the series of actions constituting this singular pantomime was accompanied by the ordinary states of consciousness, the appropriate train of ideas, or not? Did the man dream that he was skirmishing? or was he in the condition of one of Vaucauson's automata—a senseless mechanism worked by molecular changes in his nervous system? The analogy of the frog shows that the latter assumption is perfectly justifiable.

The ex-sergeant has a good voice, and had, at one time, been employed as a singer at a café. In one of his abnormal states he was observed to begin humming a tune. He then went to his room, dressed himself carefully, and took up some parts of a periodical novel, which lay on his bed, as if he were trying to find something. Dr. Mesnet, suspecting that he was seeking his music, made up one of these into a roll and put it into his

hand. He appeared satisfied, took up his cane and went down stairs to the door. Here Dr. Mesnet turned him round, and he walked quite contentedly, in the opposite direction, toward the room of the concièrge. The light of the sun shining through a window now happened to fall upon him, and seemed to suggest the footlights of the stage on which he was accustomed to make his appearance. He stopped, opened his roll of imaginary music, put himself into the attitude of a singer, and sang, with perfect execution, three songs, one after the other. After which he wiped his face with his handkerchief and drank, without a grimace, a tumbler of strong vinegar and water which was put into his hand.

An experiment which may be performed upon the frog deprived of the fore part of its brain, well known as Göltz's "Quakversuch," affords a parallel to this performance. If the skin of a certain part of the back of such a frog is gently stroked with the finger, it immediately croaks. It never croaks unless it is so stroked, and the croak always follows the stroke, just as the sound of a repeater follows the touching of the spring. In the frog, this "song" is innate—so to speak *à priori*—and depends upon a mechanism in the brain governing the vocal apparatus, which is set at work by the molecular change set up in the sensory nerves of the skin of the back by the contact of a foreign body.

In man there is also a vocal mechanism, and the cry of an infant is in the same sense innate and *à priori*, inasmuch as it depends on an organic relation between its sensory nerves and the nervous mechanism which governs the vocal apparatus. Learning to speak, and learning to sing, are processes by which the vocal mechanism is set to new tunes. A song which has been learned has its molecular equivalent, which potentially represents it in the brain, just as a musical box wound up potentially represents an overture. Touch the stop and the overture begins; send a molecular impulse along the proper afferent nerve and the singer begins his song.

Again, the manner in which the frog, though apparently insensible to light, is yet, under some circumstances, influenced by visual images, finds a singular parallel in the case of the ex-sergeant.

Sitting at a table, in one of his abnormal states, he took up a pen, felt for paper and ink, and began to write a letter to his general, in which he recommended himself for a medal, on account of his good conduct and courage. It occurred to Dr. Mesnet to ascertain experimentally how far vision was concerned in this act of writing. He therefore interposed a screen between the man's eyes and his hands; under these circumstances he went on writing for a short time, but the words became illegible, and he finally stopped, without manifesting any discontent. On the withdrawal of the screen he began to write again where he had left off. The substitution of water for ink in the inkstand had a similar result. He stopped, looked at his pen, wiped it on his coat, dipped it in the water, and began again, with the same effect.

On one occasion, he began to write upon the topmost of ten superimposed sheets of paper. After he had written a line or two, this sheet was suddenly drawn away. There was a slight expression of surprise, but he continued his letter on the second sheet exactly as if it had been the first. This operation was repeated five times, so that the fifth sheet contained nothing but the writer's signature at the bottom of the page. Nevertheless, when the signature was finished his eyes turned to the top of the blank sheet, and he went through the form of reading over what he had written, a movement of the lips accompanying each word; moreover, with his pen, he put in such corrections as were needed, in that part of the blank page which corresponded with the position of the words which required correction, in the sheets which had been taken away. If the five sheets had been transparent, therefore, they would, when superposed, have formed a properly written and corrected letter.

Immediately after he had written his letter, F—— got up, walked down to the garden, made himself a cigarette, lighted and smoked it. He was about to prepare another, but sought in vain for his tobacco-pouch, which had been purposely taken away. The pouch was now thrust before his eyes and put under his nose, but he neither saw nor smelt it; but, when it was placed in his hand, he at once seized it, made a fresh cigarette, and ignited a match to light the latter. The match was blown out and another lighted match placed close before his eyes, but he made no attempt to take it; and, if his cigarette was lighted for him, he made no attempt to smoke. All this time the eyes were vacant, and neither winked, nor exhibited any contraction of the pupils. From these and other expe-

riments, Dr. Mesnet draws the conclusion that his patient sees some things and not others; that the sense of sight is accessible to all things which are brought into relation with him by the sense of touch, and, on the contrary, insensible to things which lie outside this relation. He sees the match he holds, and does not see any other.

Just so the frog "sees" the book which is in the way of his jump, at the same time that isolated visual impressions take no effect upon him.*

As I have pointed out, it is impossible to prove that F—— is absolutely unconscious in his abnormal state, but it is no less impossible to prove the contrary; and the case of the frog goes a long way to justify the assumption that, in the abnormal state, the man is a mere insensible machine.

If such facts as these had come under the knowledge of Descartes, would they not have formed an apt commentary upon that remarkable passage in the "Traité de l'Homme," which I have quoted elsewhere,† but which is worth repetition?—

"All the functions which I have attributed to this machine (the body), as the digestion of food, the pulsation of the heart and of the arteries; the nutrition and the growth of the limbs; respiration, wakefulness, and sleep; the reception of light, sounds, odors, flavors, heat, and such like qualities, in the organs of the external senses; the impression of the ideas of these in the organ of common sensation and in the imagination; the retention or the impression of these ideas on the memory; the internal movements of the appetites and the passions; and lastly, the external movements of all the limbs, which follow so aptly, as well the action of the objects which are presented to the senses, as the impressions which meet in the memory, that they imitate as nearly as possible those of a real man; I desire, I say, that you should consider that these functions in the machine naturally proceed from the mere arrangement of its organs, neither more nor less than do the movements of a clock, or other automaton, from that of its weights and its wheels; so that, so far as these are concerned, it is not necessary to conceive any other vegetative or sensitive soul, nor any other principle of motion or of life, than the blood and the spirits agitated by the fire which burns continually in the heart, and which is no wise essentially different from all the fires which exist in inanimate bodies."

And would Descartes not have been justified in asking why we need deny that animals are machines, when men, in a state of unconsciousness, perform, mechanically, actions as complicated and as seemingly rational as those of any animals?

But though I do not think that Descartes's hypothesis can be positively refuted, I am not disposed to accept it. The doctrine of continuity is too well established for it to be permissible to me to suppose that any complex natural phenomenon comes into existence suddenly, and without being preceded by simpler modifications; and very strong arguments would be needed to prove that such complex phenomena, as those of consciousness, first make their appearance in man. We know, that, in the individual man, consciousness grows from a dim glimmer to its full light, whether we consider the infant advancing in years, or the adult emerging from slumber and swoon. We know, further, that the lower animals possess, though less developed, that part of the brain which we have every reason to believe to be the organ of consciousness in man; and as, in other cases, function and organ are proportional, so we have a right to conclude it is with the brain; and that the brutes, though they may not possess our intensity of consciousness, and though, from the absence of language, they can have no trains of thoughts, but only trains of feelings, yet have a consciousness which, more or less distinctly, foreshadows our own.

I confess that, in view of the struggle

* Those who have had occasion to become acquainted with the phenomena of somnambulism and of mesmerism, will be struck with the close parallel which they present to the proceedings of F. in his abnormal state. But the great value of Dr. Mesnet's observations lies in the fact that the abnormal condition is traceable to a definite injury to the brain, and that the circumstances are such as to keep us clear of the cloud of voluntary and involuntary fictions in which the truth is too often smothered in such cases. In the unfortunate subjects of such abnormal conditions of the brain, the disturbance of the sensory and intellectual faculties is not unfrequently accompanied by a perturbation of the moral nature, which may manifest itself in a most astonishing love of lying for its own sake. And, in this respect, also, F.'s case is singularly instructive, for though, in his normal state, he is a perfectly honest man, in his abnormal condition he is an inveterate thief, stealing and hiding away whatever he can lay hands on, with much dexterity, and with an absurd indifference as to whether the property is his own or not. Hoffman's terrible conception of the "Doppeltgänger" is realized by men in this state—who live two lives, in the one of which they may be guilty of the most criminal acts, while, in the other, they are eminently virtuous and respectable. Neither life knows anything of the other. Dr. Mesnet states that he has watched a man in his abnormal state elaborately prepare to hang himself, and has let him go on until asphyxia set in, when he cut him down. But on passing into the normal state the would-be suicide was wholly ignorant of what had happened. The problem of responsibility is here as complicated as that of the prince-bishop, who swore as a prince and not as a bishop. "But, highness, if the prince is damned what will become of the bishop?" said the peasant.

† "Lay Sermons, Essays and Reviews," p. 355.

for existence which goes on in the animal world, and of the frightful quantity of pain with which it must be accompanied, I should be glad if the probabilities were in favor of Descartes's hypothesis; but, on the other hand, considering the terrible practical consequences to domestic animals which might ensue from any error on our part, it is as well to err on the right side, if we err at all, and deal with them as weaker brethren, who are bound, like the rest of us, to pay their toll for living, and suffer what is needful for the general good. As Hartley finely says, "We seem to be in the place of God to them;" and we may justly follow the precedents He sets in nature in our dealings with them.

But though we may see reason to disagree with Descartes's hypothesis that brutes are unconscious machines, it does not follow that he was wrong in regarding them as automata. They may be more or less conscious, sensitive, automata; and the view that they are such conscious machines is that which is implicitly, or explicitly, adopted by most persons. When we speak of the actions of the lower animals being guided by instinct and not by reason, what we really mean is that, though they feel as we do, yet their actions are the results of their physical organization. We believe, in short, that they are machines, one part of which (the nervous system) not only sets the rest in motion, and co-ordinates its movements in relation with changes in surrounding bodies, but is provided with special apparatus, the function of which is the calling into existence of those states of consciousness which are termed sensations, emotions, and ideas. I believe that this generally accepted view is the best expression of the facts at present known.

It is experimentally demonstrable—any one who cares to run a pin into himself may perform a sufficient demonstration of the fact—that a mode of motion of the nervous system is the immediate antecedent of a state of consciousness. All but the adherents of "Occasionalism," or of the doctrine of "Pre-established Harmony" (if any such now exist), must admit that we have as much reason for regarding the mode of motion of the nervous system as the cause of the state of consciousness, as we have for regarding any event as the cause of another. How the one phenomenon causes the other we know, as much or as little, as in any other case of causation; but we have as much right to believe that the sensation is an effect of the molecular change, as we have to believe that motion is an effect of impact; and there is as much propriety in saying that the brain evolves sensation, as there is in saying that an iron rod, when hammered, evolves heat.

As I have endeavored to show, we are justified in supposing that something analogous to what happens in ourselves takes place in the brutes, and that the affections of their sensory nerves give rise to molecular changes in the brain, which again give rise to, or evolve, the corresponding states of consciousness. Nor can there be any reasonable doubt that the emotions of brutes, and such ideas as they possess, are similarly dependent upon molecular brain changes. Each sensory impression leaves behind a record in the structure of the brain—an "ideagenous" molecule, so to speak, which is competent, under certain conditions, to reproduce, in a fainter condition, the state of consciousness which corresponds with that sensory impression; and it is these "ideagenous molecules" which are the physical basis of memory.

It may be assumed, then, that molecular changes in the brain are the causes of all the states of consciousness of brutes. Is there any evidence that these states of consciousness may, conversely, cause those molecular changes which give rise to muscular motion? I see no such evidence. The frog walks, hops, swims, and goes through his gymnastic performances quite as well without consciousness, and consequently without volition, as with it; and, if a frog, in his natural state, possesses anything corresponding with what we call volition, there is no reason to think that it is anything but a concomitant of the molecular changes in the brain which form part of the series involved in the production of motion.

The consciousness of brutes would appear to be related to the mechanism of their body simply as a collateral product of its working, and to be as completely without any power of modifying that working as the steam-whistle which accompanies the work of a locomotive engine is without influence upon its machinery. Their volition, if they have any, is an emotion indicative of physical changes, not a cause of such changes.

This conception of the relations of states of consciousness with molecular changes in the brain—of *psychoses* with *neuroses*—does not prevent us from ascribing free will to brutes. For an agent is free when there is nothing to prevent

him from doing that which he desires to do. If a grayhound chases a hare, he is a free agent, because his action is in entire accordance with his strong desire to catch the hare; while so long as he is held back by the leash he is not free, being prevented by external force from following his inclination. And the ascription of freedom to the grayhound under the former circumstances is by no means inconsistent with the other aspect of the facts of the case—that he is a machine impelled to the chase, and caused, at the same time, to have the desire to catch the game by the impression which the rays of light proceeding from the hare make upon his eyes, and through them upon his brain.

Much ingenious argument has, at various times, been bestowed upon the question: How is it possible to imagine that volition, which is a state of consciousness, and, as such, has not the slightest community of nature with matter in motion, can act upon the moving matter of which the body is composed, as it is assumed to do in voluntary acts? But if, as is here suggested, the voluntary acts of brutes—or, in other words, the acts which they desire to perform—are as purely mechanical as the rest of their actions, and are simply accompanied by the state of consciousness called volition, the inquiry, so far as they are concerned, becomes superfluous. Their volitions do not enter into the chain of causation of their actions at all.

The hypothesis that brutes are conscious automata is perfectly consistent with any view that may be held respecting the often discussed and curious question whether they have souls or not; and, if they have souls, whether those souls are immortal or not. It is obviously harmonious with the most literal adherence to the text of Scripture concerning "the beast that perisheth;" but it is not inconsistent with the amiable conviction ascribed by Pope to his "untutored savage," that when he passes to the happy hunting-grounds in the sky, "his faithful dog shall bear him company." If the brutes have consciousness and no souls, then it is clear that, in them, consciousness is a direct function of material changes; while, if they possess immaterial subjects of consciousness or souls, then, as consciousness is brought into existence only as the consequence of molecular motion of the brain, it follows that it is an indirect product of material changes. The soul stands related to the body as the bell of a clock to the works, and consciousness answers to the sound which the bell gives out when it is struck.

Thus far I have strictly confined myself to the problem with which I proposed to deal at starting—the automatism of brutes. The question is, I believe, a perfectly open one, and I feel happy in running no risk of either Papal or Presbyterian condemnation for the views which I have ventured to put forward. And there are so very few interesting questions which one is, at present, allowed to think out scientifically—to go as far as reason leads, and stop where evidence comes to an end—without speedily being deafened by the tattoo of "the drum ecclesiastic"—that I have luxuriated in my rare freedom, and would now willingly bring this disquisition to an end if I could hope that other people would go no farther. Unfortunately, past experience debars me from entertaining any such hope, even if

". . . . . that drum's discordant sound
Parading round and round and round,"

were not, at present, as audible to me, as it was to the mild poet who ventured to express his hatred of drums in general, in that well-known couplet.

It will be said, that I mean that the conclusions deduced from the study of the brutes are applicable to man, and that the logical consequences of such application are fatalism, materialism, and atheism—whereupon the drums will beat the *pas de charge*.

One does not do battle with drummers; but I venture to offer a few remarks for the calm consideration of thoughtful persons, untrammeled by foregone conclusions, unpledged to shore-up tottering dogmas, and anxious only to know the true bearings of the case.

It is quite true that, to the best of my judgment, the argumentation which applies to brutes holds equally good of men; and, therefore, that all states of consciousness in us, as in them, are immediately caused by molecular changes of the brain-substance. It seems to me that in men, as in brutes, there is no proof that any state of consciousness is the cause of change in the motion of the matter of the organism. If these positions are well based, it follows that our mental conditions are simply the symbols in consciousness of the changes which take place automatically in the organism; and that, to take an extreme illustration, the feeling we call volition is not the cause of a vol-

untary act, but the symbol of that state of the brain which is the immediate cause of that act. We are conscious automata, endowed with free will in the only intelligible sense of that much-abused term—inasmuch as in many respects we are able to do as we like—but none the less parts of the great series of causes and effects which, in unbroken continuity, composes that which is, and has been, and shall be—the sum of existence.

As to the logical consequences of this conviction of mine, I may be permitted to remark that logical consequences are the scarecrows of fools and the beacons of wise men. The only question which any wise man can ask himself, and which any honest man will ask himself, is whether a doctrine is true or false. Consequences will take care of themselves; at most their importance can only justify us in testing with extra care the reasoning process from which they result.

So that if the view I have taken did really and logically lead to fatalism, materialism, and atheism, I should profess myself a fatalist, materialist, and atheist; and I should look upon those who, while they believed in my honesty of purpose and intellectual competency, should raise a hue and cry against me, as people who by their own admission preferred lying to truth, and whose opinions therefore were unworthy of the smallest attention.

But, as I have endeavored to explain on other occasions, I really have no claim to rank myself among fatalistic, materialistic, or atheistic philosophers. Not among fatalists, for I take the conception of necessity to have a logical, and not a physical foundation; not among materialists, for I am utterly incapable of conceiving the existence of matter if there is no mind in which to picture that existence; not among atheists, for the problem of the ultimate cause of existence is one which seems to me to be hopelessly out of reach of my poor powers. Of all the senseless babble I have ever had occasion to read, the demonstrations of these philosophers who undertake to tell us all about the nature of God would be the worst, if they were not surpassed by the still greater absurdities of the philosophers who try to prove that there is no God.

And if this personal disclaimer should not be enough, let me further point out that a great many persons whose acuteness and learning will not be contested, and whose Christian piety, and, in some cases, strict orthodoxy, are above suspicion, have held more or less definitely the view that man is a conscious automaton.

It is held, for example, in substance, by the whole school of predestinarian theologians, typified by St. Augustine, Calvin, and Jonathan Edwards—the great work of the latter on the will showing in this, as in other cases, that the growth of physical science has introduced no new difficulties of principle into theological problems, but has merely given visible body, as it were, to those which already existed.

Among philosophers, the pious Geulincx and the whole school of occasionalist Cartesians held this view: the orthodox Leibnitz invented the term "automate spirituel," and applied it to man; the fervent Christian, Hartley, was one of the chief advocates and best expositors of the doctrine; while another zealous apologist of Christianity in a sceptical age, and a contemporary of Hartley, Charles Bonnet, the Genevese naturalist, has embodied the doctrine in language of such precision and simplicity, that I will quote the little-known passage of his "Essai de Psychologie" at length:—

"ANOTHER HYPOTHESIS CONCERNING THE MECHANISM OF IDEAS.*

"Philosophers accustomed to judge of things by that which they are in themselves, and not by their relation to receive ideas, would not be shocked if they met with the proposition that the soul is a mere spectator of the movements of its body: that the latter performs of itself all that series of actions which constitutes life: that it moves of itself: that it is the body alone which reproduces ideas, compares and arranges them; which forms reasonings, imagines and executes plans of all kinds, etc. This hypothesis, though perhaps of an excessive boldness, nevertheless deserves some consideration.

"It is not to be denied that Supreme Power could create an automaton which should exactly imitate all the external and internal actions of man.

"I understand by external actions, all those movements which pass under our eyes; I term internal actions, all the motions which in the natural state cannot be observed because they take place in the interior of the body—such as the movements of digestion, circulation, sensation, etc. Moreover, I include in this category the movements which give rise to ideas, whatever be their nature.

"In the automaton which we are considering everything would be precisely determined. Everything would occur according to the rules of the most admirable mechanism: one state would succeed another state, one operation would lead to another operation, according to invariable laws; motion would become

* "Essai de Psychologie," chap. xxvii.

alternately cause and effect, effect and cause; reaction would answer to action, and reproduction to production.

"Constructed with definite relations to the activity of the beings which compose the world, the automaton would receive impressions from it, and, in faithful correspondence thereto, it would execute a corresponding series of motions.

"Indifferent toward any determination, it would yield equally to all, if the first impressions did not, so to speak, wind up the machine and decide its operations and its course.

"The series of movements which this automaton could execute would distinguish it from all others formed on the same model, but which, not having been placed in similar circumstances, would not have experienced the same impressions, or would not have experienced them in the same order.

"The senses of the automaton, set in motion by the objects presented to it, would communicate their motion to the brain, the chief motor apparatus of the machine. This would put in action the muscles of the hands and feet, in virtue of their secret connection with the senses. These muscles, alternately contracted and dilated, would approximate or remove the automaton from the objects, in the relation which they would bear to the conservation or the destruction of the machine.

"The motions of perception and sensation which the objects would have impressed on the brain, would be preserved in it by the energy of its mechanism. They would become more vivid according to the actual condition of the automaton, considered in itself and relatively to the objects.

"Words being only the motions impressed on the organ of hearing and that of voice, the diversity of these movements, their combination, the order in which they would succeed one another, would represent judgments, reasoning, and all the operations of the mind.

"A close correspondence between the organs of the senses, either by the opening into one another of their nervous ramifications, or by interposed springs (*ressorts*), would establish such a connection in their working, that, on the occasion of the movements impressed on one of these organs, other movements would be excited, or would become more vivid in some of the other senses.

"Give the automaton a soul which contemplates its movements, which believes itself to be the author of them, which has different volitions on the occasion of the different movements, and you will on this hypothesis construct a man.

"But would this man be free? Can the feeling of our liberty, this feeling which is so clear and so distinct and so vivid as to persuade us that we are the authors of our actions, be conciliated with this hypothesis? If it removes the difficulty which attends the conception of the action of the soul on the body, on the other hand it leaves untouched that which meets us in endeavoring to conceive the action of the body on the soul."

But if Leibnitz, Jonathan Edwards, and Hartley—men who rank among the giants of the world of thought—could see no antagonism between the doctrine under discussion and Christian orthodoxy, is it not just possible that smaller folk may be wrong in making such a coil about "logical consequences"? And seeing how large a share of this clamor is raised by the clergy of one denomination or another, may I say, in conclusion, that it really would be well if ecclesiastical persons would reflect that ordination, whatever deep-seated graces it may confer, has never been observed to be followed by any visible increase in the learning or the logic of its subject. Making a man a Bishop, or entrusting him with the office of ministering to even the largest of Presbyterian congregations, or setting him up to lecture to a Church congress, really does not in the smallest degree augment such title to respect as his opinions may intrinsically possess. And, when such a man presumes on an authority which was conferred upon him for other purposes, to sit in judgment upon matters his incompetence to deal with which is patent, it is permissible to ignore his sacerdotal pretensions, and to tell him, as one would tell a mere common, unconsecrated layman, that it is not necessary for any man to occupy himself with problems of this kind unless he so choose; life is filled full enough by the performance of its ordinary and obvious duties. But that, if a man elect to become a judge of these grave questions; still more, if he assume the responsibility of attaching praise or blame to his fellowmen for the conclusions at which they arrive touching them, he will commit a sin more grievous than most breaches of the Decalogue, unless he avoid a lazy reliance upon the information that is gathered by prejudice and filtered through passion, unless he go back to the prime sources of knowledge—the facts of nature, and the thoughts of those wise men who for generations past have been her best interpreters.

II.

SCIENCE AND CULTURE.*

SIX years ago, as some of my present hearers may remember, I had the privi-

* An Address delivered at the opening of Sir Josiah Mason's Science College, Birmingham (1880).

lege of addressing a large assemblage of the inhabitants of this city, who had gathered together to do honor to the memory of their famous townsman, Joseph Priestley; and, if any satisfaction attaches to posthumous glory, we may hope that the manes of the burnt-out philosopher were then finally appeased.

No man, however, who is endowed with a fair share of common sense, and not more than a fair share of vanity, will identify either contemporary or posthumous fame with the highest good; and Priestley's life leaves no doubt that he, at any rate, set a much higher value upon the advancement of knowledge, and the promotion of that freedom of thought which is at once the cause and the consequence of intellectual progress.

Hence I am disposed to think that, if Priestley could be amongst us to-day, the occasion of our meeting would afford him even greater pleasure than the proceedings which celebrated the centenary of his chief discovery. The kindly heart would be moved, the high sense of social duty would be satisfied, by the spectacle of well-earned wealth, neither squandered in tawdry luxury and vainglorious show, nor scattered with the careless charity which blesses neither him that gives nor him that takes, but expended in the execution of a well-considered plan for the aid of present and future generations of those who are willing to help themselves.

We shall all be of one mind thus far. But it is needful to share Priestley's keen interest in physical science; and to have learned, as he had learned, the value of scientific training in fields of inquiry apparently far remote from physical science; in order to appreciate, as he would have appreciated, the value of the noble gift which Sir Josiah Mason has bestowed upon the inhabitants of the Midland district.

For us children of the nineteenth century, however, the establishment of a college under the conditions of Sir Josiah Mason's Trust, has a significance apart from any which it could have possessed a hundred years ago. It appears to be an indication that we are reaching the crisis of the battle, or rather of the long series of battles, which have been fought over education in a campaign which began long before Priestley's time, and will probably not be finished just yet.

In the last century, the combatants were the champions of ancient literature, on the one side, and those of modern literature on the other; but, some thirty years* ago, the contest became complicated by the appearance of a third army, ranged round the banner of Physical Science.

I am not aware that any one has authority to speak in the name of this new host. For it must be admitted to be somewhat of a guerilla force, composed largely of irregulars, each of whom fights pretty much for his own hand. But the impressions of a full private, who has seen a good deal of service in the ranks, respecting the present position of affairs and the conditions of a permanent peace, may not be devoid of interest; and I do not know that I could make a better use of the present opportunity than by laying them before you.

From the time that the first suggestion to introduce physical science into ordinary education was timidly whispered, until now, the advocates of scientific education have met with opposition of two kinds. On the one hand, they have been pooh-poohed by the men of business who pride themselves on being the representatives of practicality; while, on the other hand, they have been excommunicated by the classical scholars, in their capacity of Levites in charge of the ark of culture and monopolists of liberal education.

The practical men believed that the idol whom they worship—rule of thumb—has been the source of the past prosperity, and will suffice for the future welfare of the arts and manufactures. They were of opinion that science is speculative rubbish; that theory and practice have nothing to do with one another; and that the scientific habit of mind is an impediment, rather than an aid, in the conduct of ordinary affairs.

I have used the past tense in speaking of the practical men—for although they were very formidable thirty years ago, I am not sure that the pure species has not been extirpated. In fact, so far as mere argument goes, they have been subjected to such a *feu d'enfer* that it is a miracle if any have escaped. But I have remarked that your typical practical man has an unexpected resemblance to one of Milton's angels. His spiritual wounds, such as are inflicted by logical weapons, may be as deep as a well and as wide as a church door, but beyond shedding a few drops of ichor, celestial or otherwise, he is no

* The advocacy of the introduction of physical science into general education by George Combe and others commenced a good deal earlier; but the movement had acquired hardly any practical force before the time to which I refer.

whit the worse. So, if any of these opponents be left, I will not waste time in vain repetition of the demonstrative evidence of the practical value of science; but knowing that a parable will sometimes penetrate where syllogisms fail to effect an entrance, I will offer a story for their consideration.

Once upon a time, a boy, with nothing to depend upon but his own vigorous nature, was thrown into the thick of the struggle for existence in the midst of a great manufacturing population. He seems to have had a hard fight, inasmuch as, by the time he was thirty years of age, his total disposable funds amounted to twenty pounds. Nevertheless, middle life found him giving proof of his comprehension of the practical problems he had been roughly called upon to solve, by a career of remarkable prosperity.

Finally, having reached old age with its well-earned surroundings of "honor, troops of friends," the hero of my story bethought himself of those who were making a like start in life, and how he could stretch out a helping hand to them.

After long and anxious reflection this successful practical man of business could devise nothing better than to provide them with the means of obtaining "sound, extensive, and practical scientific knowledge." And he devoted a large part of his wealth and five years of incessant work to this end.

I need not point the moral of a tale which, as the solid and spacious fabric of the Scientific College assures us, is no fable, nor can anything which I could say intensify the force of this practical answer to practical objections.

We may take it for granted then, that, in the opinion of those best qualified to judge, the diffusion of thorough scientific education is an absolutely essential condition of industrial progress; and that the College which has been opened to-day will confer an inestimable boon upon those whose livelihood is to be gained by the practice of the arts and manufactures of the district.

The only question worth discussion is, whether the conditions, under which the work of the College is to be carried out, are such as to give it the best possible chance of achieving permanent success.

Sir Josiah Mason, without doubt most wisely, has left very large freedom of action to the trustees, to whom he proposes ultimately to commit the administration of the College, so that they may be able to adjust its arrangements in accordance with the changing conditions of the future. But, with respect to three points, he has laid most explicit injunctions upon both administrators and teachers.

Party politics are forbidden to enter into the minds of either, so far as the work of the College is concerned; theology is as sternly banished from its precincts; and finally, it is especially declared that the College shall make no provision for "mere literary instruction and education."

It does not concern me at present to dwell upon the first two injunctions any longer than may be needful to express my full conviction of their wisdom. But the third prohibition brings us face to face with those other opponents of scientific education, who are by no means in the moribund condition of the practical man, but alive, alert, and formidable.

It is not impossible that we shall hear this express exclusion of "literary instruction and education" from a College which, nevertheless, professes to give a high and efficient education, sharply criticised. Certainly the time was that the Levites of culture would have sounded their trumpets against its walls as against an educational Jericho.

How often have we not been told that the study of physical science is incompetent to confer culture; that it touches none of the higher problems of life; and, what is worse, that the continual devotion to scientific studies tends to generate a narrow and bigoted belief in the applicability of scientific methods to the search after truth of all kinds. How frequently one has reason to observe that no reply to a troublesome argument tells so well as calling its author a "mere scientific specialist." And, as I am afraid it is not permissible to speak of this form of opposition to scientific education in the past tense; may we not expect to be told that this, not only omission, but prohibition, of "mere literary instruction and education" is a patent example of scientific narrow-mindedness?

I am not acquainted with Sir Josiah Mason's reasons for the action which he has taken; but if, as I apprehend is the case, he refers to the ordinary classical course of our schools and universities by the name of "mere literary instruction and education," I venture to offer sundry reasons of my own in support of that action.

For I hold very strongly by two convictions—The first is, that neither the discipline nor the subject-matter of classical education is of such direct value to the

student of physical science as to justify the expenditure of valuable time upon either; and the second is, that for the purpose of attaining real culture, an exclusively scientific education is at least as effectual an an exclusively literary education.

I need hardly point out to you that these opinions, especially the later, are diametrically opposed to those of the great majority of educated Englishmen, influenced as they are by school and university traditions. In their belief, culture is obtainable only by a liberal education; and a liberal education is synonymous, not merely with education and instruction in literature, but in one particular form of literature, namely, that of Greek and Roman antiquity. They hold that the man who has learned Latin and Greek, however little, is educated; while he who is versed in other branches of knowledge, however deeply, is a more or less respectable specialist, not admissible into the cultured caste. The stamp of the educated man, the University degree, is not for him.

I am too well acquainted with the generous catholicity of spirit, the true sympathy with scientific thought, which pervades the writings of our chief apostle of culture to identify him with these opinions; and yet one may cull from one and another of those epistles to the Philistines, which so much delight all who do not answer to that name, sentences which lend them some support.

Mr. Arnold tells us that the meaning of culture is "to know the best that has been thought and said in the world." It is the criticism of life contained in literature. That criticism regards "Europe as being, for intellectual and spiritual purposes, one great confederation, bound to a joint action and working to a common result; and whose members have, for their common outfit, a knowledge of Greek, Roman, and Eastern antiquity, and of one another. Special, local, and temporary advantages being put out of account, that modern nation will in the intellectual and spiritual sphere make most progress, which most thoroughly carries out this programme. And what is that but saying that we too, all of us, as individuals, the more thoroughly we carry it out, shall make the more progress?"

We have here to deal with two distinct propositions. The first, that a criticism of life is the essence of culture; the second, that literature contains the materials which suffice for the construction of such a criticism.

I think that we must all assent to the first proposition. For culture certainly means something quite different from learning or technical skill. It implies the possession of an ideal, and the habit of critically estimating the value of things by comparison with a theoretic standard. Perfect culture should apply a complete theory of life, based upon a clear knowledge alike of its possibilities and of its limitations.

But we may agree to all this, and yet strongly dissent from the assumption that literature alone is competent to supply this knowledge. After having learnt all that Greek, Roman, and Eastern antiquity have thought and said, and all that modern literatures have to tell us, it is not self-evident that we have laid a sufficiently broad and deep foundation for that criticism of life wich constitutes culture.

Indeed, to any one acquainted with the scope of physical science, it is not at all evident. Considering progress only in the "intellectual and spiritual sphere," I find myself wholly unable to admit that either nations or individuals will really advance, if their common outfit draws nothing from the stores of physical science. I should say that an army, without weapons of precision, and with no particular base of operations, might more hopefully enter upon a campaign on the Rhine, than a man, devoid of a knowledge of what physical science has done in the last century, upon a criticism of life.

When a biologist meets with an anomaly, he instinctively turns to the study of development to clear it up. The rationale of contradictory opinions may with equal confidence be sought in history.

It is, happily, no new thing that Englishmen should employ their wealth in building and endowing institutions for educational purposes. But, five or six hundred years ago, deeds of foundation expressed or implied conditions as nearly as possible contrary to those which have been thought expedient by Sir Josiah Mason. That is to say, physical science was practically ignored, while a certain literary training was enjoined as a means to the acquirement of knowledge which was essentially theological.

The reason of this singular contradiction between the actions of men alike animated by a strong and disinterested desire to promote the welfare of their fellows, is easily discovered.

At that time, in fact, if any one desired knowledge beyond such as could be obtained by his own observation, or by

common conversation, his first necessity was the learn the Latin language, inasmuch as all the higher knowledge of the western world was contained in works written in that language. Hence, Latin grammar, with logic and rhetoric, studied through Latin, were the fundamentals of education. With respect to the substance of the knowledge imparted through this channel, the Jewish and Christian Scriptures, as interpreted and supplemented by the Romish Church, were held to contain a complete and infallibly true body of information.

Theological dicta were, to the thinkers of those days, that which the axioms and definitions of Euclid are to the geometers of these. The business of the philosophers of the middle ages was to deduce from the data furnished by the theolgians, conclusions in accordance with ecclesiastical decrees. They were allowed the high privilege of showing, by logical process, how and why that which the Church said was true, must be true. And if their demonstrations fell short of or exceeded this limit, the Church was maternally ready to check their aberrations, if need be, by the help of the secular arm.

Between the two, our ancestors were furnished with a compact and complete criticism of life. They were told how the world began, and how it would end; they learned that all material existence was but a base and insignificant blot upon the fair face of the spiritual world, and that nature was, to all intents and purposes, the play-ground of the devil; they learned that the earth is the center of the visible universe, and that man is the cynosure of things terrestrial; and more especially is it inculcated that the course of nature had no fixed order, but that it could be, and constantly was, altered by the agency of innumerable spiritual beings, good and bad, according as they were moved by the deeds and prayers of men. The sum and substance of the whole doctrine was to produce the conviction that the only thing really worth knowing in this world was how to secure that place in a better which, under certain conditions, the Church promised.

Our ancestors had a living belief in this theory of life, and acted upon it in their dealings with education, as in all other matters. Culture meant saintliness —after the fashion of the saints of those days; the education that led to it was, of necessity, theological; and the way to theology lay through Latin.

That the study of nature—further than was requisite for the satisfaction of everyday wants—should have any bearing on human life was far from the thoughts of men thus trained. Indeed, as nature had been cursed for man's sake, it was an obvious conclusion that those who meddled with nature were likely to come into pretty close contact with Satan. And, if any born scientific investigator followed his instincts, he might safely reckon upon earning the reputation, and probably upon suffering the fate, of a sorcerer.

Had the western world been left to itself in Chinese isolation, there is no saying how long this state of things might have endured. But, happily, it was not left to itself. Even earlier than the thirteenth century, the development of Moorish civilization in Spain and the great movement of the Crusades had introduced the leaven which, from that day to this, has never ceased to work. At first, though the intermediation of Arabic translations, afterward by the study of the originals, the western nations of Europe became acquainted with the writings of the ancient philosophers and poets, and, in time, with the whole of the vast literature of antiquity.

Whatever there was of high intellectual aspiration or dominant capacity in Italy, France, Germany, and England, spent itself for centuries in taking possession of the rich inheritance left by the dead civilizations of Greece and Rome. Marvelously aided by the invention of printing, classical learning spread and flourished. Those who possessed it prided themselves on having attained the highest culture then within the reach of mankind.

And justly. For, saving Dante on his solitary pinnacle, there was no figure in modern literature at the time of the Renascence to compare with the men of antiquity; there was no art to compete with their sculpture; there was no physical science but that which Greece had created. Above all, there was no other example of perfect intellectual freedom— of the unhesitating acceptance of reason as the sole guide to truth and the supreme arbiter of conduct.

The new learning necessarily soon exerted a profound influence upon education. The language of the monks and schoolmen seemed little better than gibberish to scholars fresh from Virgil and Cicero, and the study of Latin was placed upon a new foundation. Moreover, Latin itself ceased to afford the sole key to

knowledge. The student who sought the highest thought of antiquity, found only a second-hand reflection of it in Roman literature, and turned his face to the full light of the Greeks. And after a battle, not altogether dissimilar to that which is at present being fought over the teaching of physical science, the study of Greek was recognized as an essential element of all higher education.

Thus the Humanists, as they were called, won the day; and the great reform which they effected was of incalculable service to mankind. But the Nemesis of all reformers is finality; and the reformers of education, like those of religion, fell into the profound, however common, error of mistaking the beginning for the end of the work of reformation.

The representatives of the Humanists in the nineteenth century, take their stand upon classical education as the sole avenue to culture, as firmly as if we were still in the age of Renascence. Yet, surely, the present intellectual relations of the modern and the ancient worlds are profoundly different from those which obtained three centuries ago. Leaving aside the existence of a great and characteristically modern literature, of modern painting, and, especially, of modern music, there is one feature of the present state of the civilized world which separates it more widely from the Renascence, than the Renascence was separated from the middle ages.

This distinctive character of our own times lies in the vast and constantly increasing part which is played by natural knowledge. Not only is our daily life shaped by it, not only does the prosperity of millions of men depend upon it, but our whole theory of life has long been influenced, consciously or unconsciously, by the general conceptions of the universe, which have been forced upon us by physical science.

In fact, the most elementary acquaintance with the results of scientific investigation shows us that they offer a broad and striking contradiction to the opinions so implicitly credited and taught in the middle ages.

The notions of the beginning and the end of the world entertained by our forefathers are no longer credible. It is very certain that the earth is not the chief body in the material universe, and that the world is not subordinated to man's use. It is even more certain that nature is the expression of a definite order with which nothing interferes, and that the chief business of mankind is to learn that order and govern themselves accordingly. Moreover this scientific "criticism of life" presents itself to us with different credentials from any other. It appeals not to authority, nor to what anybody may have thought or said, but to nature. It admits that all our interpretations of natural fact or more or less imperfect and symbolic, and bids the learner seek for truth not among words but among things. It warns us that the assertion which outstrips evidence is not only a blunder but a crime.

The purely classical education advocated by the representatives of the Humanists in our day, gives no inkling of all this. A man may be a better scholar than Erasmus, and know no more of the chief causes of the present intellectual fermentation than Erasmus did. Scholarly and pious persons, worthy of all respect, favor us with allocutions upon the sadness of the antagonism of science to their mediæval way of thinking, which betray an ignorance of the first principles of scientific investigation, an incapacity for understanding what a man of science means by veracity, and an unconsciousness of the weight of established scientific truths, which is almost comical.

There is no great force in the *tu quoque* argument, or else the advocates of scientific education might fairly enough retort upon the modern Humanists that they may be learned specialists, but that they possess no such sound foundation for a criticism of life as deserves the name of culture. And, indeed, if we were disposed to be cruel, we might urge that the Humanists have brought this reproach upon themselves, not because they are too full of the spirit of the ancient Greek, but because they lack it.

The period of the Renascence is commonly called that of the "Revival of Letters," as if the influences then brought to bear upon the mind of Western Europe has been wholly exhausted in the field of literature. I think it is very commonly forgotten that the revival of science, effected by the same agency, although less conspicuous, was not less momentous.

In fact, the few and scattered students of nature of that day picked up the clue to her secrets exactly as it fell from the hands of the Greeks a thousand years before. The foundations of mathematics were so well laid by them, that our children learn their geometry from a book written for the schools of Alexandria two thousand years ago. Modern astronomy

is the natural continuation and development of the work of Hipparchus and of Ptolemy; modern physics of that of Democritus and of Archimedes; it was long before modern biological science outgrew the knowledge bequeathed to us by Aristotle, by Theophrastus, and by Galen.

We cannot know all the best thoughts and sayings of the Greeks unless we know what they thought about natural phenomena. We cannot fully apprehend their criticism of life unless we understand the extent to which that criticism was affected by scientific conceptions. We falsely pretend to be the inheritors of their culture, unless we are penetrated, as the best minds among them were, with an unhesitating faith that the free employment of reason, in accordance with scientific method, is the sole method of reaching truth.

Thus I venture to think that the pretensions of our modern Humanists to the possession of the monopoly of culture and to the exclusive inheritance of the spirit of antiquity must be abated, if not abandoned. But I should be very sorry that anything I have said should be taken to imply a desire on my part to depreciate the value of classical education, as it might be and as it sometimes is. The native capacities of mankind vary no less than their opportunities; and while culture is one, the road by which one man may best reach it is widely different from that which is most advantageous to another. Again, while scientific education is yet inchoate and tentative, classical education is thoroughly well organized upon the practical experience of generations of teachers. So that, given ample time for learning and destination for ordinary life, or for a literary career, I do not think that a young Englishman in search of culture can do better than follow the course usually marked out for him, supplementing its deficiencies by his own efforts.

But for those who mean to make science their serious occupation; or who intend to follow the profession of medicine; or who have to enter early upon the business of life; for all these, in my opinion, classical education is a mistake; and it is for this reason that I am glad to see "mere literary education and instruction" shut out from the curriculum of Sir Josiah Mason's College, seeing that its inclusion would probably lead to the introduction of the ordinary smattering of Latin and Greek.

Nevertheless, I am the last person to question the importance of genuine literary education, or to suppose that intellectual culture can be complete without it. An exclusively scientific training will bring about a mental twist as surely as an exclusively literary training. The value of the cargo does not compensate for a ship's being out of trim; and I should be very sorry to think that the Scientific College would turn out none but lop-sided men.

There is no need, however, that such a catastrophe should happen. Instruction in English, French, and German is provided, and thus the three greatest literatures of the modern world are made accessible to the student.

French and German, and especially the latter language, are absolutely indispensable to those who desire full knowledge in any department of science. But even supposing that the knowledge of these languages acquired is not more than sufficient for purely scientific purposes, every Englishman has, in his native tongue, an almost perfect instrument of literary expression; and, in his own literature, models of every kind of literary excellence. If an Englishman cannot get literary culture out of his Bible, his Shakespeare, his Milton, neither, in my belief, will the profoundest study of Homer and Sophocles, Virgil and Horace, give it to him.

Thus, since the constitution of the College makes sufficient provision for literary as well as for scientific education, and since artistic instruction is also contemplated, it seems to me that a fairly complete culture is offered to all who are willing to take advantage of it.

But I am not sure that at this point the "practical" man, scotched but not slain, may ask what all this talk about culture has to do with an Institution, the object of which is defined to be "to promote the prosperity of the manufactures and the industry of the country." He may suggest that what is wanted for this end is not culture, nor even a purely scientific discipline, but simply a knowledge of applied science.

I often wish that this phrase, "applied science," had never been invented. For it suggests that there is a sort of scientific knowledge of direct practical use, which can be studied apart from another sort of scientific knowledge, which is of no practical utility, and which is termed "pure science." But there is no more complete fallacy than this. What people call applied science is nothing but the application of pure science to particular classes of problems. It consists of deductions

from those general principles, established by reasoning and observation, which constitute pure science. No one can safely make these deductions until he has a firm grasp of the principles; and he can obtain that grasp only by personal experience of the operations of observation and of reasoning on which they are founded.

Almost all the processes employed in the arts and manufactures fall within the range either of physics or of chemistry. In order to improve them, one must thoroughly understand them; and no one has a chance of really understanding them, unless he has obtained that mastery of principles and that habit of dealing with facts, which is given by long-continued and well-directed purely scientific training in the physical and the chemical laboratory. So that there really is no question as to the necessity of purely scientific discipline, even if the work of the College were limited by the narrowest interpretation of its stated aims.

And, as to the desirableness of a wider culture than that yielded by science alone, it is to be recollected that the improvement of manufacturing processes is only one of the conditions which contribute to the prosperity of industry. Industry is a means and not an end; and mankind work only to get something which they want. What that something is depends partly on their innate, and partly on their acquired, desires.

If the wealth resulting from prosperous industry is to be spent upon the gratification of unworthy desires, if the increasing perfection of manufacturing processes is to be accompanied by an increasing debasement of those who carry them on, I do not see the good of industry and prosperity.

Now it is perfectly true that men's views of what is desirable depend upon their characters; and that the innate proclivities to which we give that name are not touched by any amount of instruction. But it does not follow that even mere intellectual education may not, to an indefinite extent, modify the practical manifestation of the characters of men in their actions, by supplying them with motives unknown to the ignorant. A pleasure-loving character will have pleasure of some sort; but, if you give him the choice, he may prefer pleasures which do not degrade him to those which do. And this choice is offered to every man, who possesses in literary or artistic culture a never-failing source of pleasures, which are neither withered by age, nor staled by custom, nor embittered in the recollection by the pangs of self-reproach.

If the Institution opened to-day fulfills the intention of its founder, the picked intelligences among all classes of the population of this district will pass through it. No child born in Birmingham, henceforward, if he have the capacity to profit by the opportunities offered to him, first in the primary and other schools, and afterward in the Scientific College, need fail to obtain, not merely the instruction, but the culture most appropriate to the conditions of his life.

Within these walls, the future employer and the future artisan may sojourn together for awhile, and carry, through all their lives, the stamp of the influences then brought to bear upon them. Hence, it is not beside the mark to remind you, that the prosperity of industry depends not merely upon the improvement of manufacturing processes, not merely upon the ennobling of the individual character, but upon a third condition, namely, a clear understanding of the conditions of social life on the part of both the capitalist and the operative, and their agreement upon common principles of social action. They must learn that social phenoména are as much the expression of natural laws as any others; that no social arrangements can be permanent unless they harmonize with the requirements of social statics and dynamics; and that, in the nature of things, there is an arbiter whose decisions execute themselves.

But this knowledge is only to be obtained by the application of the methods of investigation adopted in physical researches to the investigation of the phenomena of society. Hence, I confess, I should like to see one addition made to the excellent scheme of education propounded for the College, in the shape of provision for the teaching of Sociology. For though we are all agreed that party politics are to have no place in the instruction of the College; yet in this country, practically governed as it is now by universal suffrage, every man who does his duty must exercise political functions. And, if the evils which are inseparable from the good of political liberty are to be checked, if the perpetual oscillation of nations between anarchy and despotism is to be replaced by the steady march of self-restraining freedom; it will be because men will gradually bring themselves to deal with political, as they now deal with scientific questions; to be as ashamed of undue haste and partisan prejudice in the

one case as in the other; and to believe that the machinery of society is at least as delicate as that of a spinning-jenny, and as little likely to be improved by the meddling of those who have not taken the trouble to master the principles of its action.

In conclusion, I am sure that I make myself the mouthpiece of all present in offering to the venerable founder of the Institution, which now commences its beneficent career, our congratulations on the completion of his work; and in expressing the conviction, that the remotest posterity will point to it as a crucial instance of the wisdom which natural piety leads all men to ascribe to their ancestors.

III.

ON ELEMENTARY INSTRUCTION IN PHYSIOLOGY.*

THE chief ground upon which I venture to recommend that the teaching of elementary physiology should form an essential part of any organized course of instruction in matters pertaining to domestic economy, is, that a knowledge of even the elements of this subject supplies those conceptions of the constitution and mode of action of the living body, and of the nature of health and disease, which prepare the mind to receive instruction from sanitary science.

It is, I think, eminently desirable that the hygienist and the physician should find something in the public mind to which they can appeal; some little stock of universally acknowledged truths, which may serve as a foundation for their warnings, and predispose toward an intelligent obedience to their recommendations.

Listening to ordinary talk about health, disease, and death, one is often led to entertain a doubt whether the speakers believe that the course of natural causation runs as smoothly in the human body as elsewhere. Indications are too often obvious of a strong, though perhaps an unavowed and half unconscious, undercurrent of opinion that the phenomena of life are not only widely different, in their superficial characters and in their practical importance, from other natural events, but that they do not follow in that definite order which characterizes the succession of all other occurrences, and the statement of which we call a law of nature.

Hence, I think, arises the want of heartiness of belief in the value of knowledge respecting the laws of health and disease, and of the foresight and care to which knowledge is the essential preliminary, which is so often noticeable; and a corresponding laxity and carelessness in practice, the results of which are too frequently lamentable.

It is said that among the many religious sects of Russia, there is one which holds that all disease is brought about by the direct and special interference of the Deity, and which, therefore, looks with repugnance, upon both preventive and curative measures as alike blasphemous interferences with the will of God. Among ourselves, the "Peculiar People" are, I believe, the only persons who hold the like doctrine in its integrity, and carry it out with logical rigor. But many of us are old enough to recollect that the administration of chloroform in assuagement of the pangs of childbirth was, at its introduction, strenuously resisted upon similar grounds.

I am not sure that the feeling, of which the doctrine to which I have referred is the full expression, does not lie at the bottom of the minds of a great many people who yet would vigorously object to give a verbal assent to the doctrine itself. However this may be, the main point is that sufficient knowledge has now been acquired of vital phenomena, to justify the assertion, that the notion, that there is anything exceptional about these phenomena, receives not a particle of support from any known fact. On the contrary, there is a vast and an increasing mass of evidence that birth and death, health and disease, are as much parts of the ordinary stream of events as the rising and setting of the sun, or the changes of the moon; and that the living body is a mechanism, the proper working of which we term health; its disturbance, disease; its stoppage, death.

The activity of this mechanism is dependent upon many and complicated conditions, some of which are hopelessly beyond our control, while others are readily accessible, and are capable of being indefinitely modified by our own actions. The business of the hygienist and of the physician is to know the range of these modifiable conditions, and how to influence them toward the maintenance of health and the prolongation of life; the business of the general public is to give an intelligent assent, and a ready obedience based upon that assent, to the rules

* Read at the Domestic Economy Congress, Birmingham (1877).

laid down for their guidance by such experts. But an intelligent assent is an assent based upon knowledge, and the knowledge which is here in question means an acquaintance with the elements of physiology.

It is not difficult to acquire such knowledge. What is true, to a certain extent, of all the physical sciences, is eminently characteristic of physiology—the difficulty of the subject begins beyond the stage of elementary knowledge, and increases with every stage of progress. While the most highly trained and the best furnished intellect may find all its resources insufficient, when it strives to reach the heights and penetrate into the depths of the problems of physiology, the elementary and fundamental truths can be made clear to a child.

No one can have any difficulty in comprehending the mechanism of circulation or respiration; or the general mode of operation of the organ of vision; though the unraveling of all the minutiæ of these processes, may, for the present, baffle the conjoined attacks of the most accomplished physicists, chemists, and mathematicians. To know the anatomy of the human body, with even an approximation to thoroughness, is the work of a life; but as much as is needed for a sound comprehension of elementary physiological truths, may be learned in a week.

A knowledge of the elements of physiology is not only easy of acquirement, but it may be made a real and practical acquaintance with the facts, as far as it goes. The subject of study is always at hand, in oneself. The principal constituents of the skeleton, and the changes of form of contracting muscles, may be felt through one's own skin. The beating of one's heart, and its connection with the pulse, may be noted; the influence of the valves of one's own veins may be shown; the movements of respiration may be observed; while the wonderful phenomena of sensation afford an endless field for curious and interesting self-study. The prick of a needle will yield, in a drop of one's own blood, material for microscopic observation of phenomena which lie at the foundation of all biological conceptions; and a cold, with its concomitant coughing and sneezing, may prove the sweet uses of adversity by helping one to a clear conception of what is meant by "reflex action."

Of course there is a limit to this physiological self-examination. But there is so close a solidarity between ourselves and our poor relations of the animal world, that our inaccessible inward parts may be supplemented by theirs. A comparative anatomist knows that a sheep's heart and lungs, or eye, must not be confounded with those of a man; but, so far as the comprehension of the elementary facts of the physiology of circulation, of respiration, and of vision goes, the one furnishes the needful anatomical data as well as the other.

Thus, it is quite possible to give instruction in elementary physiology in such a manner as, not only to confer knowledge, which, for the reason I have mentioned, is useful in itself; but to serve the purposes of a training in accurate observation, and in the methods of reasoning of physical science. But that is an advantage which I mention only incidentally, as the present Conference does not deal with education in the ordinary sense of word.

It will not be suspected that I wish to make physiologists of all the world. It would be as reasonable to accuse an advocate of the "three R's" of a desire to make an orator, an author, and a mathematician of everybody. A stumbling reader, a pot-hook writer, and an arithmetician who has not got beyond the rule of three, is not a person of brilliant acquirements; but the difference between such a member of society and one who can neither read, write, nor cipher is almost inexpressible; and no one now-a-days doubts the value of instruction, even if it goes no farther.

The saying that a little knowledge is a dangerous thing is, to my mind, a very dangerous adage. If knowledge is real and genuine, I do not believe that it is other than a very valuable possession, however infinitesimal its quantity may be. Indeed, if a little knowledge is dangerous, where is the man who has so much as to be out of danger?

If William Harvey's life-long labors had revealed to him a tenth part of that which may be made sound and real knowledge to our boys and girls, he would not only have been what he was, the greatest physiologist of his age, but he would have loomed upon the seventeenth century as a sort of intellectual portent. Our "little knowledge" would have been to him a great, astounding, unlooked-for vision of scientific truth.

I really see no harm which can come of giving our children a little knowledge of physiology. But then, as I have said, the instruction must be real, based upon ob-

servation, eked out by good explanatory diagrams and models, and conveyed by a teacher whose own knowledge has been acquired by a study of the facts; and not the mere catechismal parrot-work which too often usurps the place of elementary teaching.

It is, I hope, unnecessary for me to give a formal contradiction to the silly fiction, which is assiduously circulated by fanatics who not only ought to know, but do know, that their assertions are untrue, that I have advocated the introduction of that experimental discipline which is absolutely indispensable to the professed physiologist, into elementary teaching.

But while I should object to any experimentation which can justly be called painful, for the purpose of elementary instruction; and, while, as a member of a late Royal Commission, I gladly did my best to prevent the infliction of needless pain, for any purpose; I think it is my duty to take this opportunity of expressing my regret at a condition of the law which permits a boy to troll for pike, or set lines with live frog bait, for idle amusement; and, at the same time, lays the teacher of that boy open to the penalty of fine and imprisonment, if he uses the same animal for the purpose of exhibiting one of the most beautiful and instructive of physiological spectacles, the circulation in the web of the foot. No one could undertake to affirm that a frog is not inconvenienced by being wrapped up in a wet rag, and having his toes tied out; and it cannot be denied that inconvenience is a sort of pain. But you must not inflict the least pain on a vertebrated animal for scientific purposes (though you may do a good deal in that way for gain or for sport) without due license of the Secretary of State for the Home Department, granted under the authority of the Vivisection Act.

So it comes about, that, in this present year of grace 1877, two persons may be charged with cruelty to animals. One has impaled a frog, and suffered the creature to writhe about in that condition for hours; the other has pained the animal no more than one of us would be pained by tying strings round his fingers and keeping him in the position of a hydropathic patient. The first offender says, "I did it because I find fishing very amusing," and the magistrate bids him depart in peace; nay, probably wishes him good sport. The second pleads, "I wanted to impress a scientific truth, with a distinctness attainable in no other way, on the minds of my scholars," and the magistrate fines him five pounds.

I cannot but think that this is an anomalous and not wholly creditable state of things.

IV.

ON THE BORDER TERRITORY BETWEEN THE ANIMAL AND THE VEGETABLE KINGDOMS.*

IN the whole history of science there is nothing more remarkable than the rapidity of the growth of biological knowledge within the last half-century, and the extent of the modification which has thereby been effected in some of the fundamental conceptions of the naturalist.

In the second edition of the "Règne Animal," published in 1828, Cuvier devotes a special section to the "Division of Organized Beings into Animals and Vegetables," in which the question is treated with that comprehensiveness of knowledge and clear critical judgment which characterize his writings, and justify us in regarding them as representative expressions of the most extensive, if not the profoundest, knowledge of his time. He tells us that living beings have been subdivided from the earliest times into *animated beings*, which possess sense and motion, and *inanimated beings*, which are devoid of these functions, and simply vegetate.

Although the roots of plants direct themselves toward moisture, and their leaves toward air and light,—although the parts of some plants exhibit oscillating movements without any perceptible cause, and the leaves of others retract when touched,—yet none of these movements justify the ascription to plants of perception or of will. From the mobility of animals, Cuvier, with his characteristic partiality for teleological reasoning, deduces the necessity of the existence in them of an alimentary cavity, or reservoir of food, whence their nutrition may be drawn by the vessels which are a sort of internal roots; and, in the presence of this alimentary cavity, he naturally sees the primary and the most important distinction between animals and plants.

Following out his teleological argument, Cuvier remarks that the organization of this cavity and its appurtenances must needs vary according to the nature

* Lecture at the Royal Institution, London (1876).

of the aliment, and the operations which it has to undergo, before it can be converted into substances fitted for absorption; while the atmosphere and the earth supply plants with juices ready prepared, and which can be absorbed immediately. As the animal body required to be independent of heat and of the atmosphere, there were no means by which the motion of its fluids could be produced by internal causes. Hence arose the second great distinctive character of animals, or the circulatory system, which is less important than the digestive, since it was unnecessary, and therefore is absent, in the more simple animals.

Animals further needed muscles for locomotion and nerves for sensibility. Hence, says Cuvier, it was necessary that the chemical composition of the animal body should be more complicated than that of the plant; and it is so, inasmuch as an additional substance, nitrogen, enters into it as an essential element; while, in plants, nitrogen is only accidentally joined with the three other fundamental constituents of organic beings—carbon, hydrogen, and oxygen. Indeed, he afterward affirms that nitrogen is peculiar to animals; and herein he places the third distinction between the animal and the plant. The soil and the atmosphere supply plants with water, composed of hydrogen and oxygen; air, consisting of nitrogen and oxygen; and carbonic acid, containing carbon and oxygen. They retain the hydrogen and the carbon, exhale the superfluous oxygen, and absorb little or no nitrogen. The essential character of vegetable life is the exhalation of oxygen, which is effected through the agency of light. Animals, on the contrary, derive their nourishment either directly or indirectly from plants. They get rid of the superfluous hydrogen and carbon, and accumulate nitrogen. The relations of plants and animals to the atmosphere are therefore inverse. The plant withdraws water and carbonic acid from the atmosphere, the animal contributes both to it. Respiration—that is, the absorption of oxygen and the exhalation of carbonic acid—is the specially animal function of animals, and constitutes their fourth distinctive character.

Thus wrote Cuvier in 1828. But, in the fourth and fifth decades of this century, the greatest and most rapid revolution which biological science has ever undergone was effected by the application of the modern microscope to the investigation of organic structure; by the introduction of exact and easily manageable methods of conducting the chemical analysis of organic compounds; and finally, by the employment of instruments of precision for the measurement of the physical forces which are at work in the living economy.

That the semi-fluid contents (which we now term protoplasm) of the cells of certain plants, such as the *Charæ*, are in constant and regular motion, was made out by Bonaventura Corti a century ago; but the fact, important as it was, fell into oblivion, and had to be re-discovered by Treviranus in 1807. Robert Brown noted the more complex motions of the protoplasm in the cells of *Tradescantia* in 1831; and now such movements of the living substance of plants are well known to be some of the most widely-prevalent phenomena of vegetable life.

Agardh, and other of the botanists of Cuvier's generation, who occupied themselves with the lower plants, had observed that, under particular circumstances, the contents of the cells of certain water-weeds were set free, and moved about with considerable velocity, and with all the appearances of spontaneity, as locomotive bodies, which, from their similarity to animals of simple organization, were called "zoospores." Even as late as 1845, however, a botanist of Schleiden's eminence dealt very sceptically with these statements; and his scepticism was the more justified, since Ehrenberg, in his elaborate and comprehensive work on the *Infusoria*, had declared the greater number of what are now recognized as locomotive plants to be animals.

At the present day, innumerable plants and free plant cells are known to pass the whole or part of their lives in an actively locomotive condition, in no wise distinguishable from that of one of the simpler animals; and, while in this condition, their movements are, to all appearance, as spontaneous—as much the product of volition—as those of such animals.

Hence the teleological argument for Cuvier's first diagnostic character—the presence in animals of an alimentary cavity, or internal pocket, in which they can carry about their nutriment—has broken down, so far, at least, as his mode of stating it goes. And, with the advance of microscopic anatomy, the universality of the fact itself among animals has ceased to be predicable. Many animals of even complex structure, which live parasitically within others, are wholly devoid of an alimentary cavity. Their food

is provided for them, not only ready cooked, but ready digested, and the alimentary canal, become superfluous, has disappeared. Again, the males of most Rotifers have no digestive apparatus; as a German naturalist has remarked, they devote themselves entirely to the "Minnedienst," and are to be reckoned among the few realizations of the Byronic ideal of a lover. Finally, amidst the lowest forms of animal life, the speck of gelatinous protoplasm, which constitutes the whole body, has no permanent digestive cavity or mouth, but takes in its food anywhere; and digests, so to speak, all over its body.

But although Cuvier's leading diagnosis of the animal from the plant will not stand a strict test, it remains one of the most constant of the distinctive characters of animals. And, if we substitute for the possession of an alimentary cavity, the power of taking solid nutriment into the body and their digesting it, the definition so changed will cover all animals, except certain parasites, and the few and exceptional cases of non-parasitic animals which do not feed at all. On the other hand, the definition thus amended will exclude all ordinary vegetable organisms.

Cuvier himself practically gives up his second distinctive mark when he admits that it is wanting in the simpler animals.

The third distinction is based on a completely erroneous conception of the chemical differences and resemblances between the constituents of animal and vegetable organisms, for which Cuvier is not responsible, as it was current among contemporary chemists. It is now established that nitrogen is as essential a constituent of vegetable as of animal living matter; and that the latter is, chemically speaking, just as complicated as the former. Starchy substances, cellulose and sugar, once supposed to be exclusively confined to plants, are now known to be regular and normal products of animals. Amylaceous and saccharine substances are largely manufactured, even by the highest animals; cellulose is widespread as a constituent of the skeletons of the lower animals; and it is probable that amyloid substances are universally present in the animal organism, though not in the precise form of starch.

Moreover, although it remains true that there is an inverse relation between the green plant in sunshine and the animal, in so far as, under these circumstances, the green plant decomposes carbonic acid and exhales oxygen, while the animal absorbs oxygen and exhales carbonic acid; yet, the exact investigations of the modern chemical investigators of the physiological processes of plants have clearly demonstrated the fallacy of attempting to draw any general distinction between animals and vegetables on this ground. In fact, the difference vanishes with the sunshine, even in the case of the green plant; which, in the dark, absorbs oxygen and gives out carbonic acid like any animal.* On the other hand, those plants, such as the fungi, which contain no chlorophyll and are not green, are always, so far as respiration is concerned, in the exact position of animals. They absorb oxygen and give out carbonic acid.

Thus, by the progress of knowledge, Cuvier's fourth distinction between the animal and the plant has been as completely invalidated as the third and second; and even the first can be retained only in a modified form and subject to exceptions.

But has the advance of biology simply tended to break down old distinctions, without establishing new ones?

With a qualification, to be considered presently, the answer to this question is undoubtedly in the affirmative. The famous researches of Schwann and Schleiden in 1837 and the following years, founded the modern science of histology, or that branch of anatomy which deals with the ultimate visible structure of organisms, as revealed by the microscope; and, from that day to this, the rapid improvement of methods of investigation, and the energy of a host of accurate observers, have given greater and greater breadth and firmness to Schwann's great generalization, that a fundamental unity of structure obtains in animals and plants; and that, however diverse may be the fabrics, or *tissues*, of which their bodies are composed, all these varied structures result from the metamorphosis of morphological units (termed *cells*, in a more general sense than that in which the word "cells" was at first employed), which are not only similar in animals and in plants respectively, but present a close resemblance, when those of animals and those of plants are compared together.

The contractility which is the fundamental condition of locomotion, has not

* There is every reason to believe that living plants, like living animals, always respire, and, in respiring, absorb oxygen and give off carbonic acid; but, that in green plants exposed to daylight or to the electric light, the quantity of oxygen evolved in consequence of the decomposition of carbonic acid by a special apparatus which green plants possess exceeds that absorbed in the concurrent respiratory process.

only been discovered to exist far more widely among plants than was formerly imagined; but, in plants, the act of contraction has been found to be accompanied, as Dr. Burdon Sanderson's interesting investigations have shown, by a disturbance of the electrical state of the contractile substance, comparable to that which was found by Du Bois Reymond to be a concomitant of the activity of ordinary muscle in animals.

Again, I know of no test by which the reaction of the leaves of the Sundew and of other plants to stimuli, so fully and carefully studied by Mr. Darwin, can be distinguished from those acts of contraction following upon stimuli, which are called "reflex" in animals.

On each lobe of the bilobed leaf of Venus's fly trap (*Dionæa muscipula*) are three delicate filaments which stand out at right angle from the surface of the leaf. Touch one of them with the end of a fine human hair and the lobes of the leaf instantly close together* in virtue of an act of contraction of part of their substance, just as the body of a snail contracts into its shell when one of its "horns" is irritated.

The reflex action of the snail is the result of the presence of a nervous system in the animal. A molecular change takes place in the nerve of the tentacle, is propagated to the muscles by which the body is retracted, and causing them to contract, the act of retraction is brought about. Of course the similarity of the acts does not necessarily involve the conclusion that the mechanism by which they are effected is the same; but it suggests a suspicion of their identity which needs careful testing.

The results of recent inquiries into the structure of the nervous system of animals converge toward the conclusion that the nerve fibers, which we have hitherto regarded as ultimate elements of nervous tissue, are not such, but are simply the visible aggregations of vastly more attenuated filaments, the diameter of which dwindles down to the limits of our present microscopic vision, greatly as these have been extended by modern improvements of the microscope; and that a nerve is, in its essence, nothing but a linear tract of specially modified protoplasm between two points of an organism—one of which is able to affect the other by means of the communication so established. Hence, it is conceivable that even the simplest living being may possess a nervous system. And the question whether plants are provided with a nervous system or not, thus acquires a new aspect, and presents the histologist and physiologist with a problem of extreme difficulty, which must be attacked from a new point of view and by the aid of methods which have yet to be invented.

Thus it must be admitted that plants may be contractile and locomotive; that, while locomotive, their movements may have as much appearance of spontaneity as those of the lowest animals; and that many exhibit actions, comparable to those which are brought about by the agency of a nervous system in animals. And it must be allowed to be possible that further research may reveal the existence of something comparable to a nervous system in plants. So that I know not where we can hope to find any absolute distinction between animals and plants, unless we return to their mode of nutrition, and inquire whether certain differences of a more occult character than those imagined to exist by Cuvier, and which hold good for the vast majority of animals and plants, are of universal application.

A bean may be supplied with water in which salts of ammonia and certain other mineral salts are dissolved in due proportion; with atmospheric air containing its ordinary minute dose of carbonic acid; and with nothing else but sunlight and heat. Under these circumstances, unnatural as they are, with proper management, the bean will thrust forth its radicle and its plumule; the former will grow down into roots, the latter grow up into the stem and leaves, of a vigorous bean plant; and this plant will, in due time, flower and produce its crop of beans, just as if it were grown in the garden or in the field.

The weight of the nitrogenous protein compounds, of the oily, starchy, saccharine and woody substances contained in the full-grown plant and its seeds, will be vastly greater than the weight of the same substances contained in the bean from which it sprang. But nothing has been supplied to the bean save water, carbonic acid, ammonia, potash, lime, iron, and the like, in combination with phosphoric, sulphuric, and other acids. Neither protein, nor fat, nor starch, nor sugar, nor any substance in the slightest degree resembling them, has formed part of the food of the bean. But the weights of the carbon, hydrogen, oxygen, nitrogen, phosphorus, sulphur, and other ele-

* Darwin, "Insectivorous Plants," p. 289.

mentary bodies contained in the bean-plant, and in the seeds which it produces, are exactly equivalent to the weights of the same elements which have disappeared from the materials supplied to the bean during its growth. Whence it follows that the bean has taken in only the raw materials of its fabric, and has manufactured them into bean stuffs.

The bean has been able to perform this great chemical feat by the help of its green coloring matter, or chlorophyll; for it is only the green parts of the plant which, under the influence of sunlight, have the marvelous power of decomposing carbonic acid, setting free the oxygen and laying hold of the carbon which it contains. In fact, the bean obtains two of the absolutely indispensable elements of its substance from two distinct sources; the watery solution, in which its roots are plunged, contains nitrogen but no carbon; the air, to which the leaves are exposed, contains carbon, but its nitrogen is in the state of a free gas, in which condition the bean can make no use of it; * and the chlorophyll † is the apparatus by which the carbon is extracted from the atmospheric carbonic acid—the leaves being the chief laboratories in which this operation is effected.

The great majority of conspicuous plants are, as everybody knows, green; and this arises from the abundance of their chlorophyll. The few which contain no chlorophyll and are colorless, are unable to extract the carbon which they require from atmospheric carbonic acid, and lead a parasitic existence upon other plants; but it by no means follows, often as the statement has been repeated, that the manufacturing power of plants depends on their chlorophyll, and its interaction with the rays of the sun. On the contrary, it is easily demonstrated, as Pasteur first proved, that the lowest fungi, devoid of chlorophyll, or of any substitute for it, as they are, nevertheless possess the characteristic manufacturing powers of plants in a very high degree. Only it is necessary that they should be supplied with a different kind of raw material; as they cannot extract carbon from carbonic acid, they must be furnished with something else that contains carbon. Tartaric acid is such a substance; and if a single spore of the commonest and most troublesome of molds—*Penicillium*—be sown in a saucerful of water, in which tartrate of ammonia, with a small percentage of phosphates and sulphates is contained, and kept warm whether in the dark or exposed to light, it will, in a short time, give rise to a thick crust of mold, which contains many million times the weight of the original spore, in protein compounds and cellulose. Thus we have a very wide basis of fact for the generalization that plants are essentially characterized by their manufacturing capacity—by their power of working up mere mineral matters into complex organic compounds.

Contrariwise, there is a no less wide foundation for the generalization that animals, as Cuvier puts it, depend directly or indirectly upon plants for the materials of their bodies; that is, either they are herbivorous, or they eat other animals which are herbivorous.

But for what constituents of their bodies are animals thus dependent upon plants? Certainly not for their horny matter; nor for chondrin, the proximate chemical element of cartilage; nor for gelatine; nor for syntonin, the constituent of muscle; nor for their nervous or biliary substances; nor for their amyloid matters; nor, necessarily, for their fats.

It can be experimentally demonstrated that animals can make these for themselves. But that which they cannot make, but must, in all known cases, obtain directly or indirectly from plants, is the peculiar nitrogenous matter, protein. Thus the plant is the ideal *prolétaire* of the living world, the worker who produces; the animal, the ideal aristocrat, who mostly occupies himself in consuming, after the manner of that noble representative of the line of Zähdarm, whose epitaph is written in *Sartor Resartus*.

Here is our last hope of finding a sharp line of demarkation between plants and animals; for, as I have already hinted, there is a border territory between the two kingdoms, a sort of no-man's-land, the inhabitants of which certainly cannot be discriminated and brought to their proper allegiances in any other way.

Some months ago, Professor Tyndall asked me to examine a drop of infusion of hay, placed under an excellent and powerful microscope, and to tell him what I thought some organisms visible in it were. I looked and observed, in the first place, multitudes of *Bacteria* moving about with their ordinary intermittent

* I purposely assume that the air with which the bean is supplied in the case stated contains no ammoniacal salts.

† The recent researches of Pringsheim have raised a host of questions as to the exact share taken by chlorophyll in the chemical operations which are effected by the green parts of plants. It may be that the chlorophyll is only a constant concomitant of the actual deoxidizing apparatus.

spasmodic wriggles. As to the vegetable nature of these there is now no doubt. Not only does the close resemblance of the *Bacteria* to unquestionable plants, such as the *Oscillatoriæ*, and lower forms of *Fungi*, justify this conclusion, but the manufacturing test settles the question at once. It is only needful to add a minute drop of fluid containing *Bacteria*, to water in which tartrate, phosphate, and sulphate of ammonia are dissolved; and, in a very short space of time, the clear fluid becomes milky by reason of their prodigious multiplication, which, of course, implies the manufacture of living Bacterium-stuff out of these merely saline matters.

But other active organisms, very much larger than the *Bacteria*, attaining in fact the comparatively gigantic dimensions of one-three thousandths of an inch or more, incessantly crossed the field of view. Each of these had a body shaped like a pear, the small end being slightly incurved and produced into a long curved filament, or *cilium*, of extreme tenuity. Behind this, from the concave side of the incurvation, proceeded another long cilium, so delicate as to be discernible only by the use of the highest powers and careful management of the light. In the center of the pear-shaped body a clear round space could occasionally be discerned, but not always; and careful watching showed that this clear vacuity appeared gradually, and then shut up and disappeared suddenly, at regular intervals. Such a structure is of common occurrence among the lowest plants and animals, and is known as a *contractile vacuole*.

The little creature thus described sometimes propelled itself with great activity, with a curious rolling motion, by the lashing of the front cilium, while the second cilium trailed behind; sometimes it anchored itself by the hinder cilium and was spun round by the working of the other, its motions resembling those of an anchor buoy in a heavy sea. Sometimes, when two were in full career toward one another, each would appear dexterously to get out of the other's way; sometimes a crowd would assemble and jostle one another, with as much semblance of individual effort as a spectator on the Grands Mulets might observe with a telescope among the specks representing men in the valley of Chamounix.

The spectacle, though always surprising, was not new to me. So my reply to the question put to me was, that these organisms were what biologists call *Monads*, and though they might be animals, it was also possible that they might, like the *Bacteria*, be plants. My friend received my verdict with an expression which showed a sad want of respect for authority. He would as soon believe that a sheep was a plant. Naturally piqued by this want of faith, I have thought a good deal over the matter; and as I still rest in the lame conclusion I originally expressed, and must even now confess that I cannot certainly say whether this creature is an animal or a plant, I think it may be well to state the grounds of my hesitation at length. But in the first place, in order that I may conveniently distinguish this "Monad" from the multitude of other things which go by the same designation, I must give it a name of its own. I think (though for reasons which need not be stated at present, I am not quite sure) that it is identical with the species *Monas lens*, as defined by the eminent French microscopist Dujardin, though his magnifying power was probably insufficient to enable him to see that it is curiously like a much larger form of monad which he has named *Heteromita*. I shall, therefore, call it not *Monas*, but *Heteromita lens*.

I have been unable to devote to my *Heteromita* the prolonged study needful to work out its whole history, which would involve weeks, or it may be months, of unremitting attention. But I the less regret this circumstance, as some remarkable observations recently published by Messrs. Dallinger and Drysdale* on certain Monads, relate, in part, to a form so similar to my *Heteromita lens*, that the history of the one may be used to illustrate that of the other. These most patient and painstaking observers, who employed the highest attainable powers of the microscope and, relieving one another, kept watch day and night over the same individual monads, have been enabled to trace out the whole history of their *Heteromita*; which they found in infusions of the heads of fishes of the Cod tribe.

Of the four monads described and figured by these investigators, one, as I have said, very closely resembles *Heteromita lens* in every particular, except that it has a separately distinguishable central particle or "nucleus," which is not certainly to be made out in *Heteromita lens*; and that nothing is said by Messrs. Dallinger and Drysdale of the existence of a con-

* "Researches in the Life-history of a Cercomonad: a Lesson in Biogenesis;" and "Further Researches in the Life-history of the Monads."—*Monthly Microscopical Journal*. 1873.

tractile vacuole in this monad, though they describe it in another.

Their *Heteromita*, however, multiplied rapidly by fission. Sometimes a transverse constriction appeared, the hinder half developed a new cilium, and the hinder cilium gradually split from its base to its free end, until it was divided into two; a process which, considering the fact that this fine filament cannot be much more than one-one hundred thousandths of an inch in diameter, is wonderful enough. The constriction of the body extended inward until the two portions were united by a narrow isthmus; finally, they separated and each swam away by itself, a complete *Heteromita*, provided with its two cilia. Sometimes the constriction took a longitudinal direction, with the same ultimate result. In each case the process occupied not more than six or seven minutes. At this rate, a single *Heteromita* would give rise to a thousand like itself in the course of an hour, to about a million in two hours, and to a number greater than the generally assumed number of human beings now living in the world in three hours; or, if we give each *Heteromita* an hour's enjoyment of individual existence, the same result will be obtained in about a day. The apparent suddenness of the appearance of multitudes of such organisms as these, in any nutritive fluid to which one obtains access, is thus easily explained.

During these processes of multiplication by fission, the *Heteromita* remains active; but sometimes another mode of fission occurs. The body becomes rounded and quiescent, or nearly so; and, while in this resting state, divides into two portions, each of which is rapidly converted into an active *Heteromita*.

A still more remarkable phenomenon is that kind of multiplication which is preceded by the union of two monads, by a process which is termed *conjugation*. Two active *Heteromitæ* become applied to one another, and then slowly and gradually coalesce into one body. The two nuclei run into one; and the mass resulting from the conjugation of the two *Heteromitæ*, thus fused together, has a triangular form. The two pairs of cilia are to be seen, for some time, at two of the angles, which answer to the small ends of the conjoined monads; but they ultimately vanish, and the twin organism, in which all visible traces of organization have disappeared, falls into a state of rest. Sudden wave-like movements of its substance next occur; and, in a short time, the apices of the triangular mass burst, and give exit to a dense yellowish, glairy fluid, filled with minute granules. This process, which, it will be observed, involves the actual confluence and mixture of the substance of two distinct organisms, is effected in the space of about two hours.

The authors whom I quote say that they "cannot express" the excessive minuteness of the granules in question, and they estimate their diameter at less than one-two hundred thousandths of an inch. Under the highest powers of the microscope at present applicable such specks are hardly discernible. Nevertheless, particles of this size are massive when compared to physical molecules; whence there is no reason to doubt that each, small as it is, may have a molecular structure sufficiently complex to give rise to the phenomena of life. And, as a matter of fact, by patient watching of the place at which these infinitesimal living particles were discharged, our observers assured themselves of their growth and development into new monads. These, in about four hours from their being set free, had attained a sixth of the length of the parent, with the characteristic cilia, though at first they were quite motionless; and, in four hours more, they had attained the dimensions and exhibited all the activity of the adult. These inconceivably minute particles are therefore the germs of the *Heteromita*; and from the dimensions of these germs it is easily shown that the body formed by conjugation may, at a low estimate, have given exit to thirty thousand of them; a result of a matrimonial process whereby the contracting parties, without a metaphor, "become one flesh," enough to make a Malthusian despair of the future of the Universe.

I am not aware that the investigators from whom I have borrowed this history have endeavored to ascertain whether their monads take solid nutriment or not; so that though they help us very much to fill up the blanks in the history of my *Heteromita*, their observations throw no light on the problem we are trying to solve—Is it an animal or is it a plant?

Undoubtedly it is possible to bring forward very strong arguments in favor of regarding *Heteromita* as a plant.

For example, there is a Fungus, an obscure and almost microscopic mold, termed *Peronospora infestans*. Like many other Fungi, the *Peronosporæ* are parasitic upon other plants; and this particular *Peronospora* happens to have at-

tained much notoriety and political importance, in a way not without a parallel in the career of notorious politicians, namely, by reason of the frightful mischief it has done to mankind. For it is this *Fungus* which is the cause of the potato disease; and, therefore, *Peronospora infestans* (doubtless of exclusively Saxon origin, though not accurately known to be so) brought about the Irish famine. The plants afflicted with the malady are found to be infested by a mold, consisting of fine tubular filaments, termed *hyphæ*, which burrow through the substance of the potato plant, and appropriate to themselves the substance of their host; while, at the same time, directly or indirectly, they set up chemical changes by which even its woody framework becomes blackened, sodden, and withered.

In structure, however, the *Peronospora* is as much a mold as the common *Penicillium;* and just as the *Penicillium* multiplies by the breaking up of its hyphæ into separate rounded bodies, the spores; so, in the *Peronospora*, certain of the hyphæ grow out into the air through the interstices of the superficial cells of the potato plant, and develop spores. Each of these hyphæ usually gives off several branches. The ends of the branches dilate and become closed sacs, which eventually drop off as spores. The spores falling on some part of the same potato plant, or carried by the wind to another, may at once germinate, throwing out tubular prolongations which become hyphæ, and burrow into the substance of the plant attacked. But, more commonly, the contents of the spore divide into six or eight separate portions. The coat of the spore gives way, and each portion then emerges as an independent organism, which has the shape of a bean, rather narrower at one end than the other, convex on one side, and depressed or concave on the opposite. From the depression, two long and delicate cilia proceed, one shorter than the other, and directed forward. Close to the origin of these cilia, in the substance of the body, is a regularly pulsating, contractile vacuole. The shorter cilium vibrates actively, and effects the locomotion of the organism, while the other trails behind; the whole body rolling on its axis with its pointed end forward.

The eminent botanist, De Bary, who was not thinking of our problem, tells us, in describing the movements of these "Zoospores," that, as they swim about, "Foreign bodies are carefully avoided, and the whole movement has a deceptive likeness to the voluntary changes of place which are observed in microscopic animals."

After swarming about in this way in the moisture on the surface of a leaf or stem (which, film though it may be, is an ocean to such a fish) for half an hour, more or less, the movement of the zoospore becomes slower, and is limited to a slow turning upon its axis, without change of place. It then becomes quite quiet, the cilia disappear, it assumes a spherical form, and surrounds itself with a distinct, though delicate, membranous coat. A protuberance then grows out from one side of the sphere, and rapidly increasing in length, assumes the character of a hypha. The latter penetrates into the substance of the potato plant, either by entering a stomate, or by boring through the wall of an epidermic cell and ramifies, as a mycelium, in the substance of the plant, destroying the tissues with which it comes in contact. As these processes of multiplication take place very rapidly, millions of spores are soon set free from a single infested plant; and, from their minuteness, they are readily transported by the gentlest breeze. Since, again, the zoospores set free from each spore, in virtue of their powers of locomotion, swiftly disperse themselves over the surface, it is no wonder that the infection, once started, soon spreads from field to field, and extends its ravages over a whole country.

However, it does not enter into my present plan to treat of the potato disease, instructively as its history bears upon that of other epidemics; and I have selected the case of the *Peronospora* simply because it affords an example of an organism, which, in one stage of its existence, is truly a "Monad," indistinguishable by any important character from our *Heteromita*, and extraordinarily like it in some respects. And yet this "Monad" can be traced, step by step, through the series of metamorphoses which I have described, until it assumes the features of an organism, which is as much a plant as is an oak or an elm.

Moreover, it would be possible to pursue the analogy farther. Under certain circumstances, a process of conjugation takes place in the *Peronospora*. Two separate portions of its protoplasm become fused together, surround themselves with a thick coat, and give rise to a sort of vegetable egg called an *oospore*. After a period of rest, the contents of the oospore

break up into a number of zoospores like those already described, each of which, after a period of activity, germinates in the ordinary way. This process obviously corresponds with the conjugation and subsequent setting free of germs in the *Heteromita*.

But it may be said that the *Peronospora* is, after all, a questionable sort of plant; that it seems to be wanting in the manufacturing power, selected as the main distinctive character of vegetable life; or, at any rate, that there is no proof that it does not get its protein matter ready made from the potato plant.

Let us, therefore, take a case which is not open to these objections.

There are some small plants known to botanists as members of the genus *Coleochæte*, which, without being truly parasitic, grow upon certain water-weeds, as lichens grow upon trees. The little plant has the form of an elegant green star, the branching arms of which are divided into cells. Its greenness is due to its chlorophyll, and it undoubtedly has the manufacturing power in full degree, decomposing carbonic acid and setting oxygen free, under the influence of sunlight. But the protoplasmic contents of some of the cells of which the plant is made up occasionally divide, by a method similar to that which effects the division of the contents of the *Peronospora* spore; and the severed portions are then set free as active monad-like zoospores. Each is oval and is provided at one extremity with two long active cilia. Propelled by these, it swims about for a longer or shorter time, but at length comes to a state of rest and gradually grows into a *Coleochæte*. Moreover, as in the *Peronospora*, conjugation may take place and result in an oospore; the contents of which divide and are set free as monadiform germs.

If the whole history of the zoospores of *Peronospora* and of *Coleochæte* were unknown, they would undoubtedly be classed among "Monads" with the same right as *Heteromita*; why then may not *Heteromita* be a plant, even though the cycle of forms through which it passes shows no terms quite so complex as those which occur in *Peronospora* and *Coleochæte*? And, in fact, there are some green organisms, in every respect characteristically plants, such as *Chlamydomonas*, and the common *Volvox*, or so-called "Globe animalcule," which run through a cycle of forms of just the same simple character as those of *Heteromita*.

The name of *Chlamydomonas* is applied to certain microscopic green bodies, each of which consists of a protoplasmic central substance invested by a structureless sac. The latter contains cellulose, as in ordinary plants; and the chlorophyll which gives the green color enables the *Chlamydomonas* to decompose carbonic acid and fix carbon as they do. Two long cilia protrude through the cell-wall, and effect the rapid locomotion of this "monad," which, in all respects except its mobility, is characteristically a plant. Under ordinary circumstances, the *Chlamydomonas* multiplies by simple fission, each splitting into two or into four parts, which separate and become independent organisms. Sometimes, however, the *Chlamydomonas* divides into eight parts, each of which is provided with four instead of two cilia. These "zoospores" conjugate in pairs, and give rise to quiescent bodies, which multiply by division, and eventually pass into the active state.

Thus, so far as outward form and the general character of the cycle of modifications, through which the organism passes in the course of its life, are concerned, the resemblance between *Chlamydomonas* and *Heteromita* is of the closest description. And on the face of the matter there is no ground for refusing to admit that *Heteromita* may be related to *Chlamydomonas*, as the colorless fungus is to the green alga. *Volvox* may be compared to a hollow sphere, the wall of which is made up of coherent Chlamydomonads; and which progresses with a rotating motion effected by the paddling of the multitudinous pairs of cilia which project from its surface. Each *Volvox*-monad, moreover, possesses a red pigment spot, like the simplest form of eye known among animals. The methods of fissive multiplication and of conjugation observed in the monads of this locomotive globe are essentially similar to those observed in *Chlamydomonas*; and, though a hard battle has been fought over it, *Volvox* is now finally surrendered to the Botanists.

Thus there is really no reason why *Heteromita* may not be a plant; and this conclusion would be very satisfactory, if it were not equally easy to show that there is really no reason why it should not be an animal. For there are numerous organisms presenting the closest resemblance to *Heteromita*, and, like it, grouped under the general name of "Monads," which, nevertheless, can be observed to take in solid nutriment, and which, therefore, have a virtual, if not an actual, mouth

and digestive cavity, and thus come under Cuvier's definition of an animal. Numerous forms of such animals have been described by Ehrenberg, Dujardin, H. James Clark, and other writers on the *Infusoria*. Indeed, in another infusion of hay in which my *Heteromita lens* occurred, there were innumerable infusorial animalcules belonging to the well-known species *Colpoda cucullus*.*

Full-sized specimens of this animalcule attain a length of between one-three hundredths or one-four hundreths of an inch, so that it may have ten times the length and a thousand times the mass of a *Heteromita*. In shape, it is not altogether unlike *Heteromita*. The small end, however, is not produced into one long cilium, but the general surface of the body is covered with small actively vibrating ciliary organs, which are only longest at the small end. At the point which answers to that from which the two cilia arise in *Heteromita*, there is a conical depression, the mouth; and, in young specimens, a tapering filament, which reminds one of the posterior cilium of *Heteromita*, projects from this region.

The body consists of a soft granular protoplasmic substance, the middle of which is occupied by a large oval mass called the "nucleus;" while, at its hinder end, is a "contractile vacuole," conspicuous by its regular rhythmic appearances and disappearances. Obviously, although the *Colpoda* is not a monad, it differs from one only in subordinate details. Moreover, under certain conditions, it becomes quiescent, incloses itself in a delicate case or *cyst*, and then divides into two, four, or more portions, which are eventually set free and swim about as active *Colpodæ*.

But this creature is an unmistakable animal, and full-sized *Colpodæ* may be fed as easily as one feeds chickens. It is only needful to diffuse very finely ground carmine through the water in which they live, and, in a very short time, the bodies of the *Colpodæ* are stuffed with the deeply-colored granules of the pigment.

And if this were not sufficient evidence of the animality of *Colpoda*, there comes the fact that it is even more similar to another well-known animalcule, *Paramæcium*, than it is to a monad. But *Paramæcium* is so huge a creature compared with those hitherto discussed—it reaches one-one hundred and twentieth of an inch or more in length—that there is no difficulty in making out its organization in detail; and in proving that it is not only an animal but that it is an animal which possesses a somewhat complicated organization. For example, the surface layer of its body is different in structure from the deeper parts. There are two contractile vacuoles, from each of which radiates a system of vessel-like canals; and not only is there a conical depression continuous with a tube, which serve as mouth and gullet, but the food ingested takes a definite course, and refuse is rejected from a definite region. Nothing is easier than to feed these animals, and to watch the particles of indigo or carmine accumulate at the lower end of the gullet. From this they gradually project, surrounded by a ball of water, which at length passes with a jerk, oddly simulating a gulp, into the pulpy central substance of the body, there to circulate up one side and down the other, until its contents are digested and assimilated. Nevertheless, this complex animal multiplies by division, as the monad does, and, like the monad, undergoes conjugation. It stands in the same relation to *Heteromita* on the animal side, as *Coleochæte* does on the plant side. Start from either, and such an insensible series of gradations leads to the monad that it is impossible to say at any stage of the progress—here the line between the animal and the plant must be drawn.

There is reason to think that certain organisms which pass through a monad stage of existence, such as the *Myxomycetes*, are, at one time of their lives, dependent upon external sources for their protein matter, or are animals; and, at another period, manufacture it, or are plants. And seeing that the whole progress of modern investigation is in favor of the doctrine of continuity, it is a fair and probable speculation—though only a speculation—that, as there are some plants which can manufacture protein out of such apparently intractable mineral matters as carbonic acid, water, nitrate of ammonia, metallic and earthly salts; while others need to be supplied with their carbon and nitrogen in the somewhat less raw form of tartrate of ammonia and allied compounds; so there may be yet others, as is possibly the case with the true parasitic plants, which can only manage to put together materials still better prepared—still more nearly approximated to protein—until we arrive at such organisms as the *Psorospermiæ* and the *Pan-*

* Excellently described by Stein, almost all of whose statements I have verified.

histophyton, which are as much animal as vegetable in structure, but are animal in their dependence on other organisms for their food.

The singular circumstance observed by Meyer, that the *Torula* of yeast, though an indubitable plant, still flourishes most vigorously when supplied with the complex nitrogenous substance, pepsin; the probability that the *Peronospora* is nourished directly by the protoplasm of the potato-plant; and the wonderful facts which have recently been brought to light respecting insectivorous plants, all favor this view; and tend to the conclusion that the difference between animal and plant is one of degree rather than of kind; and that the problem whether, in a given case, an organism is an animal or a plant, may be essentially insoluble.

V.

UNIVERSITIES: ACTUAL AND IDEAL.*

ELECTED by the suffrages of your four Nations, Rector of the ancient University of which you are scholars, I take the earliest opportunity which has presented itself since my restoration to health, of delivering the Address which, by long custom, is expected of the holder of my office.

My first duty in opening that Address, is to offer you my most hearty thanks for the signal honor you have conferred upon me—an honor of which, as a man unconnected with you by personal or by national ties, devoid of political distinction, and a plebeian who stands by his order, I could not have dreamed. And it was the more surprising to me, as the five-and-twenty years which have passed over my head since I reached intellectual manhood, have been largely spent in no half-hearted advocacy of doctrines which have not yet found favor in the eyes of Academic respectability; so that, when the proposal to nominate me for your Rector came, I was almost as much astonished as was Hal o' the Wynd, "who fought for his own hand," by the Black Douglas's proffer of knighthood. And I fear that my acceptance must be taken as evidence that, less wise than the Armorer of Perth, I have not yet done with soldiering.

In fact, if, for a moment, I imagined that your intention was simply, in the kindness of your hearts, to do me honor; and that the Rector of your University, like that of some other Universities, was one of those happy beings who sit in glory for three years, with nothing to do for it save the making of a speech, a conversation with my distinguished predecessor soon dispelled the dream. I found that, by the constitution of the University of Aberdeen, the incumbent of the Rectorate is, if not a power, at any rate a potential energy; and that, whatever may be his chances of success or failure, it is his duty to convert that potential energy into a living force, directed toward such ends as may seem to him conducive to the welfare of the corporation of which he is the theoretical head.

I need not tell you that your late Lord Rector took this view of his position, and acted upon it with the comprehensive, far-seeing insight into the actual condition and tendencies, not merely of his own, but of other countries, which is his honorable characteristic among statesmen. I have already done my best, and, as long as I hold my office, I shall continue my endeavors, to follow in the path which he trod; to do what in me lies, to bring this University nearer to the ideal—alas, that I should be obliged to say ideal—of all Universities; which, as I conceive, should be places in which thought is free from all fetters; and in which all sources of knowledge, and all aids to learning, should be accessible to all comers, without distinction of creed or country, riches or poverty.

Do not suppose, however, that I am sanguine enough to expect much to come of any poor efforts of mine. If your annals take any notice of my incumbency, I shall probably go down to posterity as the Rector who was always beaten. But if they add, as I think they will, that my defeats became victories in the hands of my successors, I shall be well content.

The scenes are shifting in the great theater of the world. The act which commenced with the Protestant Reformation is nearly played out, and a wider and a deeper change than that effected three centuries ago—a reformation, or rather a revolution of thought, the extremes of which are represented by the intellectual heirs of John of Leyden and of Ignatius Loyola, rather than by those of Luther and of Leo—is waiting to come on, nay, visible behind the scenes to those who have good eyes. Men are beginning, once more, to awake to the fact that matters of belief and of speculation are of absolutely infinite practical importance; and are draw-

* Address delivered by Prof. Huxley when installed as Lord Rector of Aberdeen University (1874).

ing off from that sunny country "where it is always afternoon"—the sleepy hollow of broad indifferentism—to range themselves under their natural banners. Change is in the air. It is whirling featherheads into all sorts of eccentric orbits, and filling the steadiest with a sense of insecurity. It insists on reopening all questions and asking all institutions, however venerable, by what right they exist, and whether they are, or are not, in harmony with the real or supposed wants of mankind. And it is remarkable that these searching inquiries are not so much forced on institutions from without, as developed from within. Consummate scholars question the value of learning; priests contemn dogma; and women turn their backs upon man's ideal of perfect womanhood, and seek satisfaction in apocalyptic visions of some, as yet unrealized, epicene reality.

If there be a type of stability in this world, one would be inclined to look for it in the old Universities of England. But it has been my business of late to hear a good deal about what is going on in these famous corporations; and I have been filled with astonishment by the evidences of internal fermentation which they exhibit. If Gibbon could revisit the ancient seat of learning of which he has written so cavalierly, assuredly he would no longer speak of "the monks of Oxford sunk in prejudice and port." There, as elsewhere, port has gone out of fashion, and so has prejudice—at least that particular fine, old, crusted sort of prejudice to which the great historian alludes.

Indeed, thing are moving so fast in Oxford and Cambridge, that, for my part, I rejoiced when the Royal Commission, of which I am a member, had finished and presented the Report which related to these Universities; for we should have looked like mere plagiarists, if, in consequence of a little longer delay in issuing it, all the measures of reform we proposed had been anticipated by the spontaneous action of the Universities themselves.

A month ago I should have gone on to say that one might speedily expect changes of another kind in Oxford and Cambridge. A Commission has been inquiring into the revenues of the many wealthy societies, in more or less direct connection with the Universities, resident in those towns. It is said that the Commission has reported, and that, for the first time in recorded history, the nation, and perhaps the Colleges themselves, will know what they are worth. And it was announced that a statesman, who, whatever his other merits or defects, his aims above the level of mere party fighting, and a clear vision into the most complex practical problems, meant to deal with these revenues.

But, *Bos locutus est*. That mysterious independent variable of political calculation, Public Opinion—which some whisper is, in the present case, very much the same thing as publican's opinion—has willed otherwise. The Heads may return to their wonted slumbers—at any rate for a space.

Is the spirit of change, which is working thus vigorously in the South, likely to affect the Northern Universities, and if so, to what extent? The violence of fermentation depends, not so much on the quantity of the yeast, as on the composition of the wort, and its richness in fermentable material; and, as a preliminary to the discussion of this question, I venture to call to your minds the essential and fundamental differences between the Scottish and the English type of University.

Do not charge me with anything worse than official egotism, if I say that these differences appear to be largely symbolized by my own existence. There is no Rector in an English University. Now, the organization of the members of an University into Nations, with their elective Rector, is the last relic of the primitive constitution of Universities. The Rectorate was the most important of all offices in that University of Paris, upon the model of which the University of Aberdeen was fashioned; and which was certainly a great and flourishing institution in the twelfth century.

Enthusiasts for the antiquity of one of the two acknowledged parents of all Universities, indeed, do not hesitate to trace the origin of the "Studium Parisiense" up to that wonderful king of the Franks and Lombards, Karl, surnamed the Great, whom we all called Charlemagne, and believed to be a Frenchman, until a learned historian, by beneficent iteration, taught us better. Karl is said not to have been much of a scholar himself, but he had the wisdom of which knowledge is only the servitor. And that wisdom enabled him to see that ignorance is one of the roots of all evil.

In the Capitulary which enjoins the foundation of monasterial and cathedral schools, he says: "Right action is better than knowledge; but in order to do what is right, we must know what is right." An irrefragable truth, I fancy. Acting upon it, the king took pretty full compul-

sory powers, and carried into effect a really considerable and effectual scheme of elementary education through the length and breadth of his dominions.

No doubt the idolaters out by the Elbe, in what is now part of Prussia, objected to the Frankish king's measures; no doubt the priests, who had never hesitated about sacrificing all unbelievers in their fantastic deities and futile conjurations, were the loudest in chanting the virtues of toleration; no doubt they denounced as a cruel persecutor the man who would not allow them, however sincere they might be, to go on spreading delusions which debased the intellect, as much as they deadened the moral sense, and undermined the bonds of civil allegiance; no doubt, if they had lived in these times, they would have been able to show, with ease, that the king's proceedings were totally contrary to the best liberal principles. But it may be said, in justification of the Teutonic ruler, first, that he was born before those principles, and did not suspect that the best way of getting disorder into order was to let it alone; and, secondly, that his rough and questionable proceedings did, more or less, bring about the end he had in view. For, in a couple of centuries, the schools he sowed broadcast produced their crop of men, thirsting for knowledge and craving for culture. Such men gravitating toward Paris, as a light amidst the darkness of evil days, from Germany, from Spain, from Britain, and from Scandinavia, came together by natural affinity. By degrees they banded themselves into a society, which, as its end was the knowledge of all things knowable, called itself a "*Studium Generale*;" and when it had grown into a recognized corporation, acquired the name of "*Universitas Studii Generalis*," which, mark you, means not a "Useful Knowledge Society," but a "Knowledge-of-things-in-general Society."

And thus the first "University," at any rate on this side of the Alps, came into being. Originally it had but one Faculty, that of Arts. Its aim was to be a center of knowledge and culture; not to be, in any sense, a technical school.

The scholars seem to have studied Grammar, Logic, and Rhetoric; Arithmetic and Geometry; Astronomy; Theology; and Music. Thus, their work, however imperfect and faulty, judged by modern lights, it may have been, brought them face to face with all the leading aspects of the many-sided mind of man. For these studies did really contain, at any rate in embryo—sometimes, it may be, in caricature—what we now call Philosophy, Mathematical and Physical Science, and Art. And I doubt if the curriculum of any modern University shows so clear and generous a comprehension of what is meant by culture, as this old Trivium and Quadrivium does.

The students who had passed through the University course, and had proved themselves competent to teach, became masters and teachers of their younger brethren. Whence the distinction of Masters and Regents on the one hand, and Scholars on the other.

Rapid growth necessitated organization. The Masters and Scholars of various tongues and countries grouped themselves into four Nations; and the Nations, by their own votes at first, and subsequently by those of their Procurators, or representatives, elected their supreme head and governor, the Rector—at that time the sole representative of the University, and a very real power, who could defy Provosts interfering from without; or could inflict even corporal punishment on disobedient members within the University.

Such was the primitive constitution of the University of Paris. It is in reference to this original state of things that I have spoken of the Rectorate, and all that appertains to it, as the sole relic of that constitution.

But this original organization did not last long. Society was not then, any more than it is now, patient of culture, as such. It says to everything, "Be useful to me, or away with you." And to the learned, the unlearned man said then, as he does now, "What is the use of all your learning, unless you can tell me what I want to know? I am here blindly groping about, and constantly damaging myself by collision with three mighty powers, the power of the invisible God, the power of my fellow Man, and the power of brute Nature. Let your learning be turned to the study of these powers, that I may know how I am to comport myself with regard to them." In answer to this demand, some of the Masters of the Faculty of Arts devoted themselves to the study of Theology, some to that of Law, and some to that of Medicine; and they became Doctors—men learned in those technical, or, as we now call them, professional, branches of knowledge. Like cleaving to like, the Doctors formed schools, or Faculties, of Theology, Law, and Medicine, which sometimes assumed airs of superiority over their parent, the Faculty of Arts, though the latter

ways asserted and maintained its fundamental supremacy.

The Faculties arose by process of natural differentiation out of the primitive University. Other constituents, foreign to its nature, were speedily grafted upon it. One of these extraneous elements was forced into it by the Roman Church, which in those days asserted with effect, that which it now asserts, happily without any effect in these realms, its right of censorship and control over all teaching. The local habitation of the University lay partly in the lands attached to the monastery of S. Geneviève, partly in the diocese of the Bishop of Paris; and he who would teach must have the license of the Abbot, or of the Bishop, as the nearest representative of the Pope, so to do, which license was granted by the Chancellors of these Ecclesiastics.

Thus, if I am what archæologists call a "survival" of the primitive head and ruler of the University, your Chancellor stands in the same relation to the Papacy; and, with all respect for his Grace, I think I may say that we both look terribly shrunken when compared with our great originals.

Not so is it with a second foreign element, which silently dropped into the soil of Universities, like the grain of mustard-seed in the parable; and, like that grain, grew into a tree, in whose branches a whole aviary of fowls took shelter. That element is the element of Endowment. It differed from the preceding, in its original design to serve as a prop to the young plant, not to be a parasite upon it. The charitable and the humane, blessed with wealth, were very early penetrated by the misery of the poor student. And the wise saw that intellectual ability is not so common or so unimportant a gift that it should be allowed to run to waste upon mere handicrafts and chares. The man who was a blessing to his contemporaries, but who so often has been converted into a curse, by the blind adherence of his posterity to the letter, rather than to the spirit, of his wishes—I mean the "pious founder"—gave money and lands, that the student, who was rich in brain and poor in all else, might be taken from the plough or from the stithy, and enabled to devote himself to the higher service of mankind; and built colleges and halls in which he might be not only housed and fed, but taught.

The Colleges were very generally placed in strict subordination to the University by their founders; but, in many cases, their endowment, consisting of land, has undergone an "unearned increment," which has given these societies a continually increasing weight and importance as against the unendowed, or fixedly endowed, University. In Pharaoh's dream, the seven lean kine eat up the seven fat ones. In the reality of historical fact, the fat Colleges have eaten up the lean Universities.

Even here in Aberdeen, though the causes at work may have been somewhat different, the effects have been similar; and you see how much more substantial an entity is the Very Reverend the Principal, analogue, if not homologue, of the Principals of King's College, than the Rector, lineal representative of the ancient monarchs of the University, though now, little more than a "king of shreds and patches."

Do not suppose that, in thus briefly tracing the process of University metamorphosis, I have had any intention of quarreling with its results. Practically, it seems to me that the broad changes effected in 1858 have given the Scottish Universities a very liberal constitution, with as much real approximation to the primitive state of things as is at all desirable. If your fat kine have eaten the lean, they have not lain down to chew the cud ever since. The Scottish Universities, like the English, have diverged widely enough from their primitive model; but I cannot help thinking that the northern form has remained more faithful to its original, not only in constitution, but, what is more to the purpose, in view of the cry for change, in the practical application of the endowments connected with it.

In Aberdeen, these endowments are numerous, but so small that, taken altogether, they are not equal to the revenue of a single third-rate English college. They are scholarships, not fellowships; aids to do work—not rewards for such work as it lies within the reach of an ordinary, or even an extraordinary, young man to do. You do not think that passing a respectable examination is a fair equivalent for an income, such as many a gray-headed veteran, or clergyman, would envy; and which is larger than the endowment of many Regius chairs. You do not care to make your University a school of manners for the rich; of sports for the athletic; or a hot-bed of high-fed, hypercritical refinement, more destructive to vigor and originality than are starvation and oppression. No; your little Bursaries of ten and twenty (I believe even fifty) pounds a year, enable any boy

who has shown ability in the course of his education in those remarkable primary schools, which have made Scotland the power she is, to obtain the highest culture the country can give him; and when he is armed and equipped, his Spartan Alma Mater tells him that, so far, he has had his wages for his work, and that he may go and earn the rest.

When I think of the host of pleasant, monied, well-bred young gentlemen, who do a little learning and much boating by Cam and Isis, the vision is a pleasant one; and, as a patriot, I rejoice that the youth of the upper and richer classes of the nation receive a wholesome and a manly training, however small may be the modicum of knowledge they gather, in the intervals of this, their serious business. I admit, to the full, the social and political value of that training. But, when I proceed to consider that these young men may be said to represent the great bulk of what the Colleges have to show for their enormous wealth, plus, at least, a hundred and fifty pounds a year apiece which each undergraduate costs his parents or guardians, I feel inclined to ask, whether the rate-in-aid of the education of the wealthy and professional classes, thus levied on the resources of the community, is not, after all, a little heavy? And, still further, I am tempted to inquire what has become of the indigent scholars, the sons of the masses of the people whose daily labor just suffices to meet their daily wants, for whose benefit these rich foundations were largely, if not mainly, instituted? It seems as if Pharaoh's dream had been rigorously carried out, and that even the fat scholar has eaten the lean one. And when I turn from this picture to the no less real vision of many a brave and frugal Scotch boy, spending his summer in hard manual labor, that he may have the privilege of wending his way in autumn to this University, with a bag of oatmeal, ten pounds in his pocket, and his own stout heart to depend upon through the northern winter; not bent on seeking

"The bubble reputation at the cannon's mouth,"

but determined to wring knowledge from the hard hands of penury; when I see him win through all such outward obstacles to positions of wide usefulness and well-earned fame; I cannot but think that, in essence, Aberdeen has departed but little from the primitive intention of the founders of Universities, and that the spirit of reform has so much to do on the other side of the Border, that it may be long before he has leisure to look this way.

As compared with other actual Universities, then, Aberdeen, may, perhaps, be well satisfied with itself. But do not think me an impracticable dreamer, if I ask you not to rest and be thankful in this state of satisfaction; if I ask you to consider awhile, how this actual good stands related to that ideal better, towards which both men and institutions must progress, if they would not retrograde.

In an ideal University, as I conceive it, a man should be able to obtain instruction in all forms of knowledge, and discipline in the use of all the methods by which knowledge is obtained. In such an University, the force of living example should fire the student with a noble ambition to emulate the learning of learned men, and to follow in the footsteps of the explorers of new fields of knowledge. And the very air he breathes should be charged with that enthusiasm for truth, that fanaticism of veracity, which is a greater possession than much learning; a nobler gift than the power of increasing knowledge; by so much greater and nobler than these, as the moral nature of man is greater than the intellectual; for veracity is the heart of morality.

But the man who is all morality and intellect, although he may be good and even great, is, after all, only half a man. There is beauty in the moral world and in the intellectual world; but there is also a beauty which is neither moral nor intellectual—the beauty of the world of Art. There are men who are devoid of the power of seeing it, as there are men who are born deaf and blind, and the loss of those, as of these, is simply infinite. There are others in whom it is an overpowering passion; happy men, born with the productive, or at lowest, the appreciative, genius of the Artist. But, in the mass of mankind, the Æsthetic faculty, like the reasoning power and the moral sense, needs to be roused, directed, and cultivated; and I know not why the development of that side of his nature, through which man has access to a perennial spring of ennobling pleasure, should be omitted from any comprehensive scheme of University education.

All Universities recognize Literature in the sense of the old Rhetoric, which is art incarnate in words. Some, to their credit, recognize Art in its narrower sense, to a certain extent, and confer degrees for proficiency in some of its branches. If there are Doctors of Music, why should

there be no Masters of Painting, of Sculpture, of Architecture? I should like to see Professors of the Fine Arts in every University; and instruction in some branch of their work made a part of the Arts curriculum.

I just now expressed the opinion that, in our ideal University, a man should be able to obtain instruction in all forms of knowledge. Now, by "forms of knowledge" I mean the great classes of things knowable; of which the first, in logical, though not in natural, order is knowledge relating to the scope and limits of the mental faculties of man; a form of knowledge which, in its positive aspect, answers pretty much to Logic and part of Psychology, while, on its negative and critical side, it corresponds with Metaphysics.

A second class comprehends all that knowledge which relates to man's welfare, so far as it is determined by his own acts, or what we call his conduct. It answers to Moral and Religious philosophy. Practically, it is the most directly valuable of all forms of knowledge, but speculatively, it is limited and criticised by that which precedes and by that which follows it in my order of enumeration.

A third class embraces knowledge of the phenomena of the Universe, as that which lies about the individual man; and of the rules which those phenomena are observed to follow in the order of their occurrence, which we term the laws of Nature.

This is what ought to be called Natural Science, or Physiology, though those terms are hopelessly diverted from such a meaning; and it includes all exact knowledge of natural fact, whether Mathematical, Physical, Biological, or Social.

Kant has said that the ultimate object of all knowledge is to give replies to these three questions: What can I do? What ought I to do? What may I hope for? The forms of knowledge which I have enumerated, should furnish such replies as are within human reach, to the first and second of these questions. While to the third, perhaps the wisest answer is, "Do what you can to do what you ought, and leave hoping and fearing alone."

If this be a just and an exhaustive classification of the forms of knowledge, no question as to their relative importance, or as to the superiority of one to the other, can be seriously raised.

On the face of the matter, it is absurd to ask whether it is more important to know the limits of one's powers; or the ends for which they ought to be exerted; or the conditions under which they must be exerted. One may as well inquire which of the terms of a Rule of Three sum one ought to know, in order to get a trustworthy result. Practical life is such a sum, in which your duty multiplied into your capacity, and divided by your circumstances, gives you the fourth term in the proportion, which is your deserts, with great accuracy. All agree, I take it, that men ought to have these three kinds of knowledge. The so-called "conflict of studies" turns upon the question of how they may best be obtained.

The founders of Universities held the theory that the Scriptures and Aristotle taken together, the latter being limited by the former, contained all knowledge worth having; and that the business of philosophy was to interpret and co-ordinate these two. I imagine that in the twelfth century this was a very fair conclusion from known facts. Nowhere in the world, in those days, was there such an encyclopedia of knowledge of all three classes, as is to be found in those writings. The scholastic philosophy is a wonderful monument of the patience and ingenuity with which the human mind toiled to build up a logically consistent theory of the Universe, out of such materials. And that philosophy is by no means dead and buried, as many vainly suppose. On the contrary, numbers of men of no mean learning and accomplishment, and sometimes of rare power and subtlety of thought, hold by it as the best theory of things which has yet been stated. And, what is still more remarkable, men who speak the language of modern philosophy, nevertheless think the thoughts of the schoolmen. "The voice is the voice of Jacob, but the hands are the hands of Esau." Every day I hear "Cause," "Law," "Force," "Vitality," spoken of as entities, by people who can enjoy Swift's joke about the meat-roasting quality of the smoke-jack, and comfort themselves with the reflection that they are not even as those benighted schoolmen.

Well, this great system had its day, and then it was sapped and mined by two influences. The first was the study of classical literature, which familiarized men with methods of philosophizing; with conceptions of the highest Good; with ideas of the order of Nature; with notions of Literary and Historical Criticism; and above all, with visions of Art, of a kind which not only would not fit into the scholastic scheme, but showed them a

pre-Christian, and indeed altogether un-Christian world, of such grandeur and beauty that they ceased to think of any other. They were as men who had kissed the Fairy Queen, and wandering with her in the dim loveliness of the under-world, cared not to return to the familiar ways of home and fatherland, though they lay, at arm's length, overhead. Cardinals were more familiar with Virgil than with Isaiah; and Popes labored, with great success, to re-paganize Rome.

The second influence was the slow, but sure, growth of the physical sciences. It was discovered that some results of speculative thought, of immense practical and theoretical importance, can be verified by observation; and are always true, however severely they may be tested. Here, at any rate, was knowledge, to the certainty of which no authority could add, or take away, one jot or tittle, and to which the tradition of a thousand years was as insignificant as the hearsay of yesterday. To the scholastic system, the study of classical literature might be inconvenient and distracting, but it was possible to hope that it could be kept within bounds. Physical science, on the other hand, was an irreconcilable enemy, to be excluded at all hazards. The College of Cardinals has not distinguished itself in Physics or Physiology; and no Pope has, as yet, set up public laboratories in the Vatican.

People do not always formulate the beliefs on which they act. The instinct of fear and dislike is quicker than the reasoning process; and I suspect that, taken in conjunction with some other causes, such instinctive aversion is at the bottom of the long exclusion of any serious discipline in the physical sciences from the general curriculum of Universities; while, on the other hand, classical literature has been gradually made the backbone of the Arts course.

I am ashamed to repeat here what I have said elsewhere, in season and out of season, respecting the value of Science as knowledge and discipline. But the other day I met with some passages in the Address to another Scottish University, of a great thinker, recently lost to us, which express so fully, and yet so tersely, the truth in this matter, that I am fain to quote them:—

"To question all things;—never to turn away from any difficulty; to accept no doctrine either from ourselves or from other people without a rigid scrutiny by negative criticism; letting no fallacy, or incoherence, or confusion of thought step by unperceived; above all, to insist upon having the meaning of a word clearly understood before using it, and the meaning of a proposition before assenting to it;—these are the lessons we learn" from workers in Science. "With all this vigorous management of the negative element, they inspire no scepticism about the reality of truth or indifference to its pursuit. The noblest enthusiasm, both for the search after truth and for applying it to its highest uses, pervades those writers." "In cultivating, therefore," science as an essential ingredient in education, "we are all the while laying an admirable foundation for ethical and philosophical culture." *

The passages I have quoted were uttered by John Stuart Mill; but you cannot hear inverted commas, and it is therefore right that I should add, without delay, that I have taken the liberty of substituting "workers in science" for "ancient dialecticians," and "Science as an essential ingredient in education" for "the ancient languages as our best literary education." Mill did, in fact, deliver a noble panegyric upon classical studies. I do not doubt its justice, nor presume to question its wisdom. But I venture to maintain that no wise or just judge, who has a knowledge of the facts, will hesitate to say that it applies with equal force to scientific training.

But it is only fair to the Scottish Universities to point out that they have long understood the value of Science as a branch of general education. I observe, with the greatest satisfaction, that candidates for the degree of Master of Arts in this University are required to have a knowledge, not only of Mental and Moral Philosophy, and of Mathematics and Natural Philosophy, but of Natural History, in addition to the ordinary Latin and Greek course; and that a candidate may take honors in these subjects and in Chemistry.

I do not know what the requirements of your examiners may be, but I sincerely trust they are not satisfied with a mere book knowledge of these matters. For my own part, I would not raise a finger, if I could thereby introduce mere book work in science into every Arts curriculum in the country. Let those who want to study books devote themselves to Literature, in which we have the perfection of books, both as to substance and as to form.

* Inaugural Address delivered to the University of St. Andrews, February 1, 1867, by J. S. Mill, Rector of the University (pp. 32, 33).

If I may paraphrase Hobbes's well-known aphorism, I would say that "books are the money of Literature, but only the counters of Science," Science (in the sense in which I now use the term) being the knowledge of fact, of which every verbal description is but an incomplete and symbolic expression. And be assured that no teaching of science is worth anything, as a mental discipline, which is not based upon direct perception of the facts, and practical exercise of the observing and logical faculties upon them. Even in such a simple matter as the mere comprehension of form, ask the most practiced and widely informed anatomist what is the difference between his knowledge of a structure which he has read about, and his knowledge of the same structure when he has seen it for himself; and he will tell you that the two things are not comparable—the difference is infinite. Thus I am very strongly inclined to agree with some learned schoolmasters who say that, in their experience, the teaching of science is all waste time. As they teach it, I have no doubt it is. But to teach it otherwise, requires an amount of personal labor and a development of means and appliances, which must strike horror and dismay into a man accustomed to mere book work; and who has been in the habit of teaching a class of fifty without much strain upon his energies. And this is one of the real difficulties in the way of the introduction of physical science into the ordinary University course, to which I have alluded. It is a difficulty which will not be overcome, until years of patient study have organized scientific teaching as well as, or I hope better than, classical teaching has been organized hitherto.

A little while ago, I ventured to hint a doubt as to the perfection of some of the arrangements in the ancient Universities of England; but, in their provision for giving instruction in Science as such, and without direct reference to any of its practical applications, they have set a brilliant example. Within the last twenty years, Oxford alone has sunk more than a hundred and twenty thousand pounds in building and furnishing Physical, Chemical, and Physiological Laboratories, and a magnificent Museum, arranged with an almost luxurious regard for the needs of the student. Cambridge, less rich, but aided by the munificence of her Chancellor, is taking the same course; and, in a few years, it will be for no lack of the means and appliances of sound teaching, if the mass of English University men remain in their present state of barbarous ignorance of even the rudiments of scientific culture.

Yet another step needs to be made before Science can be said to have taken its proper place in the Universities. That is its recognition as a Faculty, or branch of study demanding recognition and special organization, on account of its bearing on the wants of mankind. The Faculties of Theology, Law, and Medicine, are technical schools, intended to equip men who have received general culture, with the special knowledge which is needed for the proper performance of the duties of clergymen, lawyers, and medical practitioners.

When the material well-being of the country depended upon rude pasture and agriculture, and still ruder mining; in the days when all the innumerable applications of the principles of physical science to practical purposes were non-existent even as dreams; days which men living may have heard their fathers speak of; what little physical science could be seen to bear directly upon human life, lay within the province of Medicine. Medicine was the foster-mother of Chemistry, because it has to do with the preparation of drugs and the detection of poisons; of Botany, because it enabled the physician to recognize medicinal herbs; of Comparative Anatomy and Physiology, because the man who studied Human Anatomy and Physiology for purely medical purposes was led to extend his studies to the rest of the animal world.

Within my recollection, the only way in which a student could obtain anything like a training in Physical Science, was by attending the lectures of the Professors of Physical and Natural science attached to the Medical Schools. But, in the course of the last thirty years, both foster-mother and child have grown so big, that they threaten not only to crush one another, but to press the very life out of the unhappy student who enters the nursery; to the great detriment of all three.

I speak in the presence of those who know practically what medical education is; for I may assume that a large proportion of my hearers are more or less advanced students of medicine. I appeal to the most industrious and conscientious among you, to those who are most deeply penetrated with a sense of the extremely serious responsibilities which attach to the calling of a medical practitioner, when I ask whether, out of the four years which you devote to your studies, you ought to spare even so much as an hour for any

work which does not tend directly to fit you for your duties?

Consider what that work is. Its foundation is a sound and practical acquaintance with the structure of the human organism, and with the modes and conditions of its action in health. I say a sound and practical acquaintance, to guard against the supposition that my intention is to suggest that you ought all to be minute anatomists and accomplished physiologists. The devotion of your whole four years to Anatomy and Physiology alone, would be totally insufficient to attain that end. What I mean is, the sort of practical, familiar, finger-end knowledge which a watchmaker has of a watch, and which you expect that craftsman, as an honest man, to have, when you entrust a watch that goes badly, to him. It is a kind of knowledge which is to be acquired, not in the lecture-room, nor in the library, but in the dissecting-room and the laboratory. It is to be had, not by sharing your attention between these and sundry other subjects, but by concentrating your minds, week after week, and month after month, six or seven hours a day, upon all the complexities of organ and function, until each of the greater truths of anatomy and physiology has become an organic part of your minds—until you would know them if you were roused and questioned in the middle of the night, as a man knows the geography of his native place and the daily life of his home. That is the sort of knowledge which, once obtained, is a life-long possession. Other occupations may fill your minds—it may grow dim, and seem to be forgotten—but there it is, like the inscription on a battered and defaced coin, which comes out when you warm it.

If I had the power to remodel Medical Education, the first two years of the medical curriculum should be devoted to nothing but such thorough study of Anatomy and Physiology, with Physiological Chemistry and Physics; the student should then pass a real, practical examination in these subjects; and, having gone through that ordeal satisfactorily, he should be troubled no more with them. His whole mind should then be given with equal intentness, to Therapeutics, in its broadest sense, to Practical Medicine and to Surgery, with instruction in Hygiene and in Medical Jurisprudence; and of these subjects only—surely there are enough of them—should he be required to show a knowledge in his final examination.

I cannot claim any special property in this theory of what the medical curriculum should be, for I find that views, more or less closely approximating these, are held by all who have seriously considered the very grave and pressing question of Medical Reform; and have, indeed, been carried into practice, to some extent, by the most enlightened Examining Boards. I have heard but two kinds of objections to them. There is, first, the objection of vested interests, which I will not deal with here, because I want to make myself as pleasant as I can, and no discussions are so unpleasant as those which turn on such points. And there is, secondly, the much more respectable objection, which takes the general form of the reproach that, in thus limiting the curriculum, we are seeking to narrow it. We are told that the medical man ought to be a person of good education and general information, if his profession is to hold its own among other professions; that he ought to know Botany, or else, if he goes abroad, he will not be able to tell poisonous fruits from edible ones; that he ought to know drugs, as a druggist knows them, or he will not be able to tell sham bark and senna from the real articles; that he ought to know Zoology, because—well, I really have never been able to learn exactly why he is to be expected to know zoology. There is, indeed, a popular superstition, that doctors know all about things that are queer or nasty to the general mind, and may, therefore, be reasonably expected to know the "barbarous binomials" applicable to snakes, snails, and slugs; an amount of information with which the general mind is usually completely satisfied. And there is a scientific superstition that Physiology is largely aided by Comparative Anatomy—a superstition which, like most superstitions, once had a grain of truth at bottom; but the grain has become homœpathic, since Physiology took its modern experimental development, and became what it is now, the application of the principles of Physics and Chemistry to the elucidation of the phenomena of life.

I hold as strongly as any one can do, that the medical practitioner ought to be a person of education and good general culture; but I also hold by the old theory of a Faculty, that a man should have his general culture before he devotes himself to the special studies of that Faculty; and I venture to maintain, that, if the general culture obtained in the Faculty of Arts were what it ought to be, the student would have quite as much knowledge of

the fundamental principles of Physics, of Chemistry, and of Biology, as he needs, before he commenced his special medical studies.

Moreover, I would urge, that a thorough study of Human Physiology is, in itself, an education broader and more comprehensive than much that passes under that name. There is no side of the intellect which it does not call into play, no region of human knowledge into which either its roots, or its branches, do not extend; like the Atlantic between the Old and the New Worlds, its waves wash the shores of the two worlds of matter and of mind; its tributary streams flow from both; through its waters, as yet unfurrowed by the keel of any Columbus, lies the road, if such there be, from the one to the other; far away from that North-west Passage of mere speculation in which so many brave souls have been hopelessly frozen up.

But whether I am right or wrong about all this, the patent fact of the limitation of time remains. As the song runs:—

> "If a man could be sure
> That his life would endure
> For the space of a thousand long years——"

he might do a number of things not practicable under present conditions. Methuselah might, with much propriety, have taken half a century to get his doctor's degree; and might, very fairly, have been required to pass a practical examination upon the contents of the British Museum, before commencing practice as a promising young fellow of two hundred, or thereabouts. But you have four years to do your work in, and are turned loose, to save or slay, at two or three and twenty.

Now, I put it to you, whether you think that, when you come down to the realities of life—when you stand by the sick-bed, racking your brains for the principles which shall furnish you with the means of interpreting symptoms, and forming a rational theory of the condition of your patient, it will be satisfactory for you to find that those principles are not there—although, to use the examination slang which is unfortunately too familiar to me, you can quite easily "give an account of the leading peculiarities of the *Marsupialia*," or "enumerate the chief characters of the *Compositæ*," or "state the class and order of the animal from which Castoreum is obtained."

I really do not think that state of things will be satisfactory to you; I am very sure it will not be so to your patient. Indeed, I am so narrow-minded myself, that if I had to choose between two physicians—one who did not know whether a whale is a fish or not, and could not tell gentian from ginger, but did understand the applications of the institutes of medicine to his art; while the other, like Talleyrand's doctor, "knew everything, even a little physic"—with all my love for breadth of culture, I should assuredly consult the former.

It is not pleasant to incur the suspicion of an inclination to injure or depreciate particular branches of knowledge. But the fact that one of those which I should have no hesitation in excluding from the medical curriculum, is that to which my own life has been specially devoted, should, at any rate, defend me from the suspicion of being urged to this course by any but the very gravest considerations of the public welfare.

And I should like, further, to call your attention to the important circumstance that, in thus proposing the exclusion of the study of such branches of knowledge as Zoology and Botany, from those compulsory upon the medical student, I am not, for a moment, suggesting their exclusion from the University. I think that sound and practical instruction in the elementary facts and broad principles of Biology should form part of the Arts Curriculum: and here, happily, my theory is in entire accordance with your practice. Moreover, as I have already said, I have no sort of doubt that, in view of the relation of Physical Science to the practical life of the present day, it has the same right as Theology, Law, and Medicine, to a Faculty of its own in which men shall be trained to be professional men of science. It may be doubted whether Universities are the places for technical schools of Engineering, or Applied Chemistry, or Agriculture. But there can surely be little question, that instruction in the branches of Science which lie at the foundation of these Arts, of a far more advanced and special character than could, with any propriety, be included in the ordinary Arts Curriculum, ought to be obtainable by means of a duly organized Faculty of Science in every University.

The establishment of such a Faculty would have the additional advantage of providing, in some measure, for one of the greatest wants of our time and country. I mean the proper support and encouragement of original research.

The other day, an emphatic friend of

mine committed himself to the opinion that, in England, it is better for a man's worldly prospects to be a drunkard, than to be smitten with the divine dipsomania of the original investigator. I am inclined to think he was not far wrong. And, be it observed, that the question is not, whether such a man shall be able to make as much out of his abilities as his brother, of like ability, who goes into Law, or Engineering, or Commerce; it is not a question of "maintaining a due number of saddle horses," as George Eliot somewhere puts it—it is a question of living or starving.

If a student of my own subject shows power and originality, I dare not advise him to adopt a scientific career; for, supposing he is able to maintain himself until he has attained distinction, I cannot give him the assurance that any amout of proficiency in the Biological Sciences will be convertible into, even the most modest, bread and cheese. And I believe that the case is as bad, or perhaps worse, with other branches of Science. In this respect Britain, whose immense wealth and prosperity hang upon the thread of Applied Science, is far behind France, and infinitely behind Germany.

And the worst of it is, that it is very difficult to see one's way to any immediate remedy for this state of affairs which shall be free from a tendency to become worse than the disease.

Great schemes for the Endowment of Research have been proposed. It has been suggested, that Laboratories for all branches of Physical Science, provided with every apparatus needed by the investigator, shall be established by the State: and shall be accessible, under due conditions and regulations, to all properly qualified persons. I see no objection to the principle of such a proposal. If it be legitimate to spend great sums of money on public Libraries and public collections of Painting and Sculpture, in aid of the men of letters, or the Artist, or for the mere sake of affording pleasure to the general public, I apprehend that it cannot be illegitimate to do as much for the promotion of scientific investigation. To take the lowest ground as a mere investment of money, the latter is likely to be much more immediately profitable. To my mind, the difficulty in the way of such schemes is not theoretical, but practical. Given the laboratories, how are the investigators to be maintained? What career is open to those who have been thus encouraged to leave bread-winning pursuits? If they are to be provided for by endowment, we come back to the College Fellowship system, the results of which, for Literature, have not been so brilliant that one would wish to see it extended to Science; unless some much better securities, than at present exist, can be taken that it will foster real work. You know that among the Bees, it depends on the kind of cell in which the egg is deposited, and the quantity and quality of food which is supplied to the grub, whether it shall turn out a busy little worker or a big idle queen. And, in the human hive, the cells of the endowed larvæ are always tending to enlarge, and their food to improve, until we get queens, beautiful to behold, but which gather no honey and build no comb.

I do not say that these difficulties may not be overcome, but their gravity is not to be lightly estimated.

In the mean while, there is one step in the direction of the endowment of research which is free from such objections. It is possible to place the scientific inquirer in a position in which he shall have ample leisure and opportunity for original work, and yet shall give a fair and tangible equivalent for those privileges. The establishment of a Faculty of Science in every University, implies that of a corresponding number of Professorial chairs, the incumbents of which need not be so burdened with teaching as to deprive them of ample leisure for original work. I do not think that it is any impediment to an original investigator to have to devote a moderate portion of his time to lecturing, or superintending practical instruction. On the contrary, I think it may be, and often is, a benefit to be obliged to take a comprehensive survey of your subject; or to bring your results to a point, and give them, as it were, a tangible objective existence. The besetting sins of the investigator are two: the one is the desire to put aside a subject, the general bearings of which he has mastered himself, and pass on to something which has the attraction of novelty; and the other, the desire for too much perfection, which leads him to

> "Add and alter many times,
> Till all be ripe and rotten;"

to spend the energies which should be reserved for action, in whitening the decks and polishing the guns.

But supposing the Professorial forces of our University to be duly organized, there remains an important question, re-

lating to the teaching power, to be considered. Is the Professorial system—the system, I mean, of teaching in the lecture-room alone, and leaving the student to find his own way when he is outside the lecture-room—adequate to the wants of learners? In answering this question, I confine myself to my own province, and I venture to reply for Physical Science, assuredly and undoubtedly, No. As I have already intimated, practical work in the Laboratory is absolutely indispensable, and that practical work must be guided and superintended by a sufficient staff of Demonstrators, who are for Science what Tutors are for other branches of study. And there must be a good supply of such Demonstrators. I doubt if the practical work of more than twenty students can be properly superintended by one Demonstrator. If we take the working day at six hours, that is less than twenty minutes apiece—not a very large allowance of time for helping a dull man, for correcting an inaccurate one, or even for making an intelligent student clearly apprehend what he is about. And, no doubt, the supplying of a proper amount of this tutorial, practical teaching, is a difficulty in the way of giving proper instruction in Physical Science in such Universities as that of Aberdeen, which are devoid of endowments; and, unlike the English Universities, have no claim on the funds of richly endowed bodies to supply their wants.

Examination—thorough, searching examination—is an indispensable accompaniment of teaching; but I am almost inclined to commit myself to the very heterodox proposition that it is a necessary evil. I am a very old Examiner, having, for some twenty years past, been occupied with examinations on a considerable scale, of all sorts and conditions of men, and women too,—from the boys and girls of elementary schools to the candidates for Honors and Fellowships in the Universities. I will not say that, in this case as in so many others, the adage, that familiarity breeds contempt, holds good; but my admiration for the existing system of examination and its products, does not wax warmer as I see more of it. Examination, like fire, is a good servant, but a bad master; and there seems to be some danger of its becoming our master. I by no means stand alone in this opinion. Experienced friends of mine do not hesitate to say that students whose career they watch, appear to them to become deteriorated by the constant effort to pass this or that examination, just as we hear of men's brains becoming affected by the daily necessity of catching a train. They work to pass, not to know; and outraged Science takes her revenge. They do pass, and they don't know. I have passed sundry examinations in my time, not without credit, and I confess I am ashamed to think how very little real knowledge underlay the torrent of stuff which I was able to pour out on paper. In fact, that which examination, as ordinarily conducted, tests, is simply a man's power of work under stimulus, and his capacity for rapidly and clearly producing that which, for the time, he has got into his mind. Now, these faculties are by no means to be despised. They are of great value in practical life, and are the making of many an advocate, and of many a so-called statesman. But in the pursuit of truth, scientific or other, they count for very little, unless they are supplemented by that long-continued, patient "intending of the mind," as Newton phrased it, which makes very little show in Examinations. I imagine that an Examiner who knows his students personally, must not unfrequently have found himself in the position of finding A's paper better than B's, though his own judgment tells him, quite clearly, that B is the man who has the larger share of genuine capacity.

Again, there is a fallacy about Examiners. It is commonly supposed that any one who knows a subject is competent to teach it; and no one seems to doubt that any one who knows a subject is competent to examine in it. I believe both these opinions to be serious mistakes: the latter, perhaps, the more serious of the two. In the first place, I do not believe that any one who is not, or has not been, a teacher is really qualified to examine advanced students. In the second place, Examination is an Art, and a difficult one, which has to be learned like all other arts.

Beginners always set too difficult questions—partly because they are afraid of being suspected of ignorance if they set easy ones, and partly from not understanding their business. Suppose that you want to test the relative physical strength of a score of young men. You do not put a hundredweight down before them, and tell each to swing it round. If you do, half of them won't be able to lift it at all, and only one or two will be able to perform the task. You must give them half a hundredweight, and see how they maneuver that, if you want to form any estimate of the muscular strength of

each. So, a practiced Examiner will seek for information respecting the mental vigor and training of candidates from the way in which they deal with questions easy enough to let reason, memory, and method have free play.

No doubt, a great deal is to be done by the careful selection of Examiners, and by the copious introduction of practical work, to remove the evils inseparable from examination; but, under the best of circumstances, I believe that examination will remain but an imperfect test of knowledge, and a still more imperfect test of capacity, while it tells next to nothing about a man's power as an investigator.

There is much to be said in favor of restricting the highest degress in each Faculty, to those who have shown evidence of such original power, by prosecuting a research under the eye of the Professor in whose province it lies; or, at any rate, under conditions which shall afford satisfactory proof that the work is theirs. The notion may sound revolutionary, but it is really very old; for, I take it, that it lies at the bottom of that presentation of a thesis by the candidate for a doctorate, which has now, too often, become little better than a matter of form.

Thus far, I have endeavored to lay before you, in a too brief and imperfect manner, my views respecting the teaching half—the Magistri and Regentes—of the University of the Future. Now let me turn to the learning half—the Scholares.

If the Universities are to be the sanctuaries of the highest culture of the country, those who would enter that sanctuary, must not come with unwashed hands. If the good seed is to yield its hundredfold harvest, it must not be scattered amidst the stones of ignorance, or the tares of undisciplined indolence and wantonness. On the contrary, the soil must have been carefully prepared, and the Professor should find that the operations of clod-crushing, draining, and weeding, and even a good deal of planting, have been done by the Schoolmaster.

That is exactly what the Professor does not find in any University in the three Kingdoms that I can hear of—the reason of which state of things lies in the extremely faulty organization of the majority of secondary Schools. Students come to the Universities ill-prepared in classics and mathematics, not at all prepared in anything else; and half their time is spent in learning that which they ought to have known when they came.

I sometimes hear it said that the Scottish Universities differ from the English, in being to a much greater extent places of comparatively elementary education for a younger class of students. But it would seem doubtful if any great difference of this kind really exists; for a high authority, himself Head of an English College, has solemnly affirmed that: "Elementary teaching of youths under twenty is now the only function performed by the University;" and that Colleges are "boarding schools in which the elements of the learned languages are taught to youth." *

This is not the first time that I have quoted those remarkable assertions. I should like to engrave them in public view, for they have not been refuted; and I am convinced that if their import is once clearly apprehended, they will play no mean part when the question of University reorganization, with a view to practical measures, comes on for discussion. You are not responsible for this anomalous state of affairs now; but, as you pass into active life and acquire the political influence to which your education and your position should entitle you, you will become responsible for it, unless each in his sphere does his best to alter it, by insisting on the improvement of secondary Schools.

Your present responsibility is of another, though not less serious, kind. Institutions do not make men, any more than organization makes life; and even the ideal University we have been dreaming about will be but a superior piece of mechanism, unless each student strive after the ideal of the Scholar. And that ideal, it seems to me, has never been better embodied than by the great Poet, who, though lapped in luxury, the favorite of a Court, and the idol of his countrymen, remained through all the length of his honored years a Scholar in Art, in Science, and in Life.

Would'st shape a noble life? Then cast
No backward glances toward the past:
And though somewhat be lost and gone,
Yet do thou act as one new-born.
What each day needs, that shalt thou ask;
Each day will set its proper task.
Give other's work just share of praise;
Not of thine own the merits raise.
Beware no fellow man thou hate:
And so in God's hands leave thy fate.†

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* "Suggestions for Academical Organization, with Especial Reference to Oxford." By the Rector of Lincoln.

† Goethe, *Zahme Xenien, Vierte Abtheilung* I should be glad to take credit for the close and vigorous English version; but it is my wife's, and not mine.

TECHNICAL EDUCATION.

BY

THOMAS H. HUXLEY.

TECHNICAL EDUCATION.

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BY THOMAS H. HUXLEY, F.R.S.

I.

TECHNICAL EDUCATION.*

ANY candid observer of the phenomena of modern society will readily admit that bores must be classed among the enemies of the human race; and a little consideration will probably lead him to the further admission, that no species of that extensive genus of noxious creatures is more objectionable than the educational bore. Convinced as I am of the truth of this great social generalization, it is not without a certain trepidation that I venture to address you on an educational topic. For, in the course of the last ten years, to go back no farther, I am afraid to say how often I have ventured to speak of education, from that given in the primary schools to that which is to be had in the universities and medical colleges; indeed, the only part of this wide region into which, as yet, I have not adventured is that into which I propose to intrude to-day.

Thus, I cannot but be aware that I am dangerously near becoming the thing which all men fear and fly. But I have deliberately elected to run the risk. For when you did me the honor to ask me to address you, an unexpected circumstance had led me to occupy myself seriously with the question of technical education; and I had acquired the conviction that there are few subjects respecting which it is more important for all classes of the community to have clear and just ideas than this; while, certainly, there is none which is more deserving of attention by the Working Men's Club and Institute Union.

It is not for me to express an opinion whether the considerations, which I am about to submit to you, will be proved by experience to be just or not; but I will do my best to make them clear. Among the many good things to be found in Lord Bacon's works, none is more full of wisdom than the saying that "truth more easily comes out of error than out of confusion." Clear and consecutive wrong-thinking is the next best thing to right-thinking; so that, if I succeed in clearing your ideas on this

* Address to the Working Men's Club, London.

topic, I shall have wasted neither your time nor my own.

"Technical education," in the sense in which the term is ordinarily used, and in which I am now employing it, means that sort of education which is specially adapted to the needs of men whose business in life it is to pursue some kind of handicraft; it is, in fact, a fine Greco-Latin equivalent for what in good vernacular English would be called "the teaching of handicrafts." And probably, at this stage of our progress, it may occur to many of you to think of the story of the cobbler and his last, and to say to yourselves, though you will be too polite to put the question openly to me, What does the speaker know practically about this matter? What is his handicraft? I think the question is a very proper one, and unless I were prepared to answer it, I hope satisfactorily, I should have chosen some other theme.

The fact is, I am, and have been, any time these thirty years, a man who works with his hands—a handicraftsman. I do not say this in the broadly metaphorical sense in which fine gentlemen, with all the delicacy of Agag about them, trip to the hustings about election time, and protest that they too are working men. I really mean my words to be taken in their direct, literal, and straightforward sense. In fact, if the most nimble-fingered watchmaker among you will come to my workshop, he may set me to put a watch together, and I will set him to dissect, say, a blackbeetle's nerves. I do not wish to vaunt, but I am inclined to think that I shall manage my job to his satisfaction sooner than he will do his piece of work to mine.

In truth, anatomy, which is my handicraft, is one of the most difficult kinds of mechanical labor, involving, as it does, not only lightness and dexterity of hand, but sharp eyes and endless patience. And you must not suppose that my particular branch of science is especially distinguished for the demand it makes upon skill in manipulation. A similar requirement is made upon all students of physical science. The astronomer, the electrician, the chemist, the mineralogist, the botanist, are constantly called upon to perform manual operations of exceeding delicacy. The progress of all branches of physical science depends upon observation, or on that artificial observation which is termed experiment, of one kind or another; and, the farther we advance, the more practical difficulties surround the investigation of the conditions of the problems offered to us; so that mobile and yet steady hands, guided by clear vision, are more and more in request in the workshops of science.

Indeed, it has struck me that one of the grounds of that sympathy between the handicraftsmen of this country and the men of science, by which it has so often been my good fortune to profit, may, perhaps, lie here. You feel and we feel that, among the so-called learned folks, we alone are brought into contact with tangible facts in the way that you are. You know well enough that it is one thing to write a history of chairs in general, or to address a poem to a throne, or to speculate about the occult powers of the chair of St. Peter; and quite another thing to make with your own hands a veritable chair, that will stand fair and square, and afford a safe and satisfactory resting-place to a frame of sensitiveness and solidity.

So it is with us, when we look out from our scientific handicrafts upon the doings of our learned brethren, whose work is untrammeled by anything "base and mechanical," as handicrafts used to be called when the world was younger, and in some respects less wise than now. We take the greatest interest in their pursuits; We are edified by their histories and are charmed with their poems, which sometimes illustrate so remarkably the powers of man's imagination; some of us admire and even humbly try to follow them in their high philosophical excursions, though we

know the risk of being snubbed by the inquiry whether groveling dissectors of monkeys and blackbeetles can hope to enter into the empyreal kingdom of speculation. But still we feel that our business is different; humbler if you will, though the diminution of dignity is, perhaps, compensated by the increase of reality; and that we, like you, have to get our work done in a region where little avails, if the power of dealing with practical tangible facts is wanting. You know that clever talk touching joinery will not make a chair; and I know that it is of about as much value in the physical sciences. Mother Nature is serenely obdurate to honeyed words; only those who understand the ways of things, and can silently and effectually handle them, get any good out of her.

And now, having, as I hope, justified my assumption of a place among handicraftsmen, and put myself right with you as to my qualification, from practical knowledge, to speak about technical education, I will proceed to lay before you the results of my experience as a teacher of a handicraft, and tell you what sort of education I should think best adapted for a boy whom one wanted to make a professional anatomist.

I should say, in the first place, let him have a good English elementary education. I do not mean that he shall be able to pass in such and such a standard—that may or may not be an equivalent expression—but that his teaching shall have been such as to have given him command of the common implements of learning and to have created a desire for the things of the understanding.

Further, I should like him to know the elements of physical science, and especially of physics and chemistry, and I should take care that this elementary knowledge was real. I should like my aspirant to be able to read a scientific treatise in Latin, French, or German, because an enormous amount of anatomical knowledge is locked up in those languages. And especially, I should require some ability to draw—I do not mean artistically, for that is a gift which may be cultivated but cannot be learned, but with fair accuracy. I will not say that everybody can learn even this; for the negative development of the faculty of drawing in some people is almost miraculous. Still everybody, or almost everybody, can learn to write; and, as writing is a kind of drawing, I suppose that the majority of the people who say they cannot draw, and give copious evidence of the accuracy of their assertion, could draw, after a fashion, if they tried. And that "after a fashion" would be better than nothing for my purposes.

Above all things, let my imaginary pupil have preserved the freshness and vigor of youth in his mind as well as his body. The educational abomination of desolation of the present day is the stimulation of young people to work at high pressure by incessant competitive examinations. Some wise man (who probably was not an early riser) has said of early risers in general, that they are conceited all the forenoon and stupid all the afternoon. Now whether this is true of early risers in the common acceptation of the word or not, I will not pretend to say; but it is too often true of the unhappy children who are forced to rise too early in their classes. They are conceited all the forenoon of life, and stupid all its afternoon. The vigor and freshness, which should have been stored up for the purposes of the hard struggle for existence in practical life, have been washed out of them by precocious mental debauchery—by book gluttony and lesson bibbing. Their faculties are worn out by the strain put upon their callow brains, and they are demoralized by worthless childish triumphs before the real work of life begins. I have no compassion for sloth, but youth has more need for intellectual rest than age; and the cheerfulness, the tenacity of purpose, the power of work which make many a successful man what he is, must often be placed

to the credit, not of his hours of industry, but to that of his hours of idleness, in boyhood. Even the hardest worker of us all, if he has to deal with anything above mere details, will do well, now and again, to let his brain lie fallow for a space. The next crop of thought will certainly be all the fuller in the ear and the weeds fewer.

This is the sort of education which I should like any one who was going to devote himself to my handicraft to undergo. As to knowing anything about anatomy itself, on the whole I would rather he left that alone until he took it up seriously in my laboratory. It is hard work enough to teach, and I should not like to have superadded to that the possible need of unteaching.

Well, but, you will say, this is Hamlet with the Prince of Denmark left out; your "technical education" is simply a good education, with more attention to physical science, to drawing, and to modern languages, than is common, and there is nothing specially technical about it.

Exactly so; that remark takes us straight to the heart of what I have to say; which is, that, in my judgment, the preparatory education of the handicraftsman ought to have nothing of what is ordinarily understood by "technical" about it.

The workshop is the only real school for a handicraft. The education which precedes that of the workshop should be entirely devoted to the strengthening of the body, the elevation of the moral faculties, and the cultivation of the intelligence; and, especially, to the imbuing the mind with a broad and clear view of the laws of that natural world with the components of which the handicraftsman will have to deal. And, the earlier the period of life at which the handicraftsman has to enter into actual practice of his craft, the more important is it that he should devote the precious hours of preliminary education to things of the mind, which have no direct and immediate bearing on his branch of industry, though they lie at the foundation of all realities.

Now let me apply the lessons I have learned from my handicraft to yours. If any of you were obliged to take an apprentice, I suppose you would like to get a good healthy lad, ready and willing to learn, handy, and with his fingers not all thumbs, as the saying goes. You would like that he should read, write, and cipher well; and, if you were an intelligent master, and your trade involved the application of scientific principles, as so many trades do, you would like him to know enough of the elementary principles of science to understand what was going on. I suppose that, in nine trades out of ten, it would be useful if he could draw; and many of you must have lamented your inability to find out for yourselves what foreigners are doing or have done. So that some knowledge of French and German might, in many cases, be very desirable.

So it appears to me that what you want is pretty much what I want; and the practical question is, How you are to get what you need, under the actual limitations and conditions of life of handicraftsmen in this country.

I think I shall have the assent both of the employers of labor and of the employed as to one of these limitations; which is, that no scheme of technical education is likely to be seriously entertained which will delay the entrance of boys into working life, or prevent them from contributing toward their own support, as early as they do at present. Not only do I believe that any such scheme could not be carried out, but I doubt its desirableness, even if it were practicable.

The period between childhood and manhood is full of difficulties and dangers, under the most favorable circumstances; and, even among the well-to-do, who can afford to surround their children with the most favorable conditions, examples of a career ruined, before it has well begun, are but too frequent. Moreover, those

who have to live by labor must be shaped to labor early. The colt that is left at grass too long makes but a sorry draught-horse, though his way of life does not bring him within the reach of artificial temptations. Perhaps the most valuable result of all education is the ability to make yourself do the thing you have to do, when it ought to be done, whether you like it or not; it is the first lesson that ought to be learned; and, however early a man's training begins, it is probably the last lesson that he learns thoroughly.

There is another reason, to which I have already adverted, and which I would reiterate, why any extension of the time devoted to ordinary school-work is undesirable. In the newly awakened zeal for education, we run some risk of forgetting the truth that while under-instruction is a bad thing, over-instruction may possibly be a worse.

Success in any kind of practical life is not dependent solely, or indeed chiefly, upon knowledge. Even in the learned professions, knowledge, alone, is of less consequence than people are apt to suppose. And, if much expenditure of bodily energy is involved in the day's work, mere knowledge is of still less importance when weighed against the probable cost of its acquirement. To do a fair day's work with his hands, a man needs, above all things, health, strength, and the patience and cheerfulness which, if they do not always accompany these blessings, can hardly in the nature of things exist without them; to which we must add honesty of purpose and a pride in doing what is done well.

A good handicraftsman can get on very well without genius, but he will fare badly without a reasonable share of that which is a more useful possession for workaday life, namely, mother-wit; and he will be all the better for a real knowledge, however limited, of the ordinary laws of nature, and especially of those which apply to his own business.

Instruction carried so far as to help the scholar to turn his store of mother-wit to account, to acquire a fair amount of sound elementary knowledge, and to use his hands and eyes; while leaving him fresh, vigorous, and with a sense of the dignity of his own calling, whatever it may be, if fairly and honestly pursued, cannot fail to be of invaluable service to all those who come under its influence.

But, on the other hand, if school instruction is carried so far as to encourage bookishness; if the ambition of the scholar is directed, not to the gaining of knowledge, but to the being able to pass examinations successfully; especially if encouragement is given to the mischievous delusion that brainwork is, in itself, and apart from its quality, a nobler or more respectable thing than handiwork—such education may be a deadly mischief to the workman, and lead to the rapid ruin of the industries it is intended to serve.

I know that I am expressing the opinion of some of the largest as well as the most enlightened employers of labor, when I say that there is a real danger that, from the extreme of no education, we may run to the other extreme of over-education of handicraftsmen. And I apprehend that what is true for the ordinary hand-worker is true for the foreman. Activity, probity, knowledge of men, ready mother-wit, supplemented by a good knowledge of the general principles involved in his business, are the making of a good foreman. If he possess these qualities, no amount of learning will fit him better for his position; while the course of life and the habit of mind required for the attainment of such learning may, in various direct and indirect ways, act as direct disqualifications for it.

Keeping in mind, then, that the two things to be avoided are, the delay of the entrance of boys into practical life, and the substitution of exhausted bookworms for shrewd, handy men, in our works and factories, let us consider what may be wisely and safely

attempted in the way of improving the education of the handicraftsman.

First, I look to the elementary schools now happily established all over the country. I am not going to criticise or find fault with them; on the contrary, their establishment seems to me to be the most important and the most beneficial result of the corporate action of the people in our day. A great deal is said of British interests just now, but, depend upon it, that no Eastern difficulty needs our intervention as a nation so seriously, as the putting down both the Bashi-Bazouks of ignorance and the Cossacks of sactarianism at home. What has already been achieved in these directions is a great thing; you must have lived some time to know how great. An education, better in its processes, better in its substance, than that which was accessible to the great majority of well-to-do Britons a quarter of a century ago, is now obtainable by every child in the land. Let any man of my age go into an ordinary elementary school, and, unless he was unusually fortunate in his youth, he will tell you that the educational method, the intelligence, patience, and good temper on the teacher's part, which are now at the disposal of the veriest waifs and wastrels of society, are things of which he had no experience in those costly middle-class schools, which were so ingeniously contrived as to combine all the evils and shortcomings of the great public schools with none of their advantages. Many a man, whose so-called education cost a good deal of valuable money and occupied many a year of invaluable time, leaves the inspection of a well-ordered elementary school devoutly wishing that, in his young days, he had had the chance of being as well taught as these boys and girls are.

But while in view of such an advance in general education, I willingly obey the natural impulse to be thankful, I am not willing altogether to rest. I want to see instruction in elementary science and in art more thoroughly incorporated in the educational system. At present, it is being administered by driblets, as if it were a potent medicine, "a few drops to be taken occasionally in a teaspoon." Every year I notice that that earnest and untiring friend of yours and of mine, Sir John Lubbock, stirs up the Government of the day in the House of Commons on this subject; and also that, every year, he and the few members of the House of Commons, such as Mr. Playfair, who sympathize with him, are met with expressions of warm admiration for science in general, and reasons at large for doing nothing in particular. But now that Mr. Forster, to whom the education of the country owes so much, has announced his conversion to the right faith, I begin to hope that, sooner or later, things will mend.

I have given what I believe to be a good reason for the assumption, that the keeping at school of boys who are to be handicraftsmen, beyond the age of thirteen or fourteen is neither practicable nor desirable; and, as it is quite certain, that with justice to other and no less important branches of education, nothing more than the rudiments of science and art teaching can be introduced into elementary schools, we must seek elsewhere for a supplementary training in these subjects, and, if need be, in foreign languages, which may go on after the workman's life has begun.

The means of acquiring the scientific and artistic part of this training already exists in full working order, in the first place, in the classes of the Science and Art Department, which are, for the most part, held in the evening, so as to be accessible to all who choose to avail themselves of them after working hours. The great advantage of these classes is that they bring the means of instruction to the doors of the factories and workshops; that they are no artificial creations, but by their very existence prove the desire of the people for them; and finally, that they admit of indefinite development in proportion as they are wanted. I have often expressed the

opinion, and I repeat it here, that, during the eighteen years they have been in existence, these classes have done incalculable good; and I can say, of my own knowledge, that the Department spares no pains and trouble in trying to increase their usefulness and ensure the soundness of their work.

No one knows better than my friend Colonel Donnelly, to whose clear views and great administrative abilities so much of the successful working of the science classes is due, that there is much to be done before the system can be said to be thoroughly satisfactory. The instruction given needs to be made more systematic and especially more practical; the teachers are of very unequal excellence, and not a few stand much in need of instruction themselves, not only in the subjects which they teach, but in the objects for which they teach. I daresay you have heard of that proceeding, reprobated by all true sportsmen, which is called "shooting for the pot." Well, there is such a thing as "teaching for the pot"—teaching, that is, not that your scholar may know, but that he may count for payment among those who pass the examination; and there are some teachers, happily not many, who have yet to learn that the examiners of the Department regard them as poachers of the worst description.

Without presuming in any way to speak in the name of the Department, I think I may say, as a matter which has come under my own observation, that it is doing its best to meet all these difficulties. It systematically promotes practical instruction in the classes; it affords facilities to teachers who desire to learn their business thoroughly; and it is always ready to aid in the suppression of pot-teaching.

All this is, as you may imagine, highly satisfactory to me. I see that spread of scientific education, about which I have so often permitted myself to worry the public, become, for all practical purposes, an accomplished fact. Grateful as I am for all that is now being done, in the same direction, in our higher schools and universities, I have ceased to have any anxiety about the wealthier classes. Scientific knowledge is spreading by what the alchemists called a "distillatio per ascensum;" and nothing now can prevent it from continuing to distil upward and permeate English society until, in the remote future, there shall be no member of the legislature who does not know as much of science as an elementary school-boy; and even the heads of houses in our venerable seats of learning shall acknowledge that natural science is not merely a sort of University backdoor through which inferior men may get at their degrees. Perhaps this apocalyptic vision is a little wild; and I feel I ought to ask pardon for an outbreak of enthusiasm, which, I assure you, is not my commonest failing.

I have said that the Government is already doing a great deal in aid of that kind of technical education for handicraftsmen which, to my mind, is alone worth seeking. Perhaps it is doing as much as it ought to do, even in this direction. Certainly there is another kind of help of the most important character, for which we may look elsewhere than to the Government. The great mass of mankind have neither the liking, or the aptitude, for either literary, or scientific, or artistic pursuits; nor, indeed, for excellence of any sort. Their ambition is to go through life with moderate exertion and a fair share of ease, doing common things in a common way. And a great blessing and comfort it is that the majority of men are of this mind; for the majority of things to be done are common things, and are quite well enough done when commonly done. The great end of life is not knowledge but action. What men need is, as much knowledge as they can assimilate and organize into a basis for action; give them more and it may become injurious. One knows people who are as heavy and stupid from undigested learning as others

are from overfulness of meat and drink. But a small percentage of the population is born with that most excellent quality, a desire for excellence, or with special aptitudes of some sort or another; Mr. Galton tells us that not more than one in four thousand may be expected to attain distinction, and not more than one in a million, some share of that intensity of instinctive aptitude, that burning thirst for excellence, which is called genius.

Now, the most important object of all educational schemes is to catch these exceptional people, and turn them to account for the good of society. No man can say where they will crop up; like their opposites, the fools and knaves, they appear sometimes in the palace, and sometimes in the hovel; but the great thing to be aimed at, I was almost going to say the most important end of all social arrangements, is to keep these glorious sports of Nature from being either corrupted by luxury or starved by poverty, and to put them into the position in which they can do the work for which they are specially fitted.

Thus, if a lad in an elementary school showed signs of special capacity, I would try to provide him with the means of continuing his education after his daily working life had begun; if, in the evening classes, he developed special capabilities in the direction of science or of drawing, I would try to secure him an apprenticeship to some trade in which those powers would have applicability. Or, if he chose to become a teacher, he should have the chance of so doing. Finally, to the lad of genius, the one in a million, I would make accessible the highest and most complete training the country could afford. Whatever that might cost depend upon it the investment would be a good one. I weigh my words when I say that if the nation could purchase a potential Watt, or Davy, or Faraday, at the cost of a hundred thousand pounds down, he would be dirt-cheap at the money. It is a mere commonplace and everyday piece of knowledge, that what these three men did has produced untold millions of wealth, in the narrowest economical sense of the word.

Therefore, as the sum and crown of what is to be done for technical education, I look to the provision of a machinery for winnowing out the capacities and giving them scope. When I was a member of the London School Board, I said, in the course of a speech, that our business was to provide a ladder, reaching from the gutter to the university, along which every child in the three kingdoms should have the chance of climbing as far as he was fit to go. This phrase was so much bandied about at the time, that, to say the truth, I am rather tired of it, but I know of no other which so fully expresses my belief, not only about education in general, but about technical education in particular.

The essential foundation of all the organization needed for the promotion of education among handicraftsmen will, I believe, exist in this country, when every working lad can feel that society has done as much as lies in its power to remove all needless and artificial obstacles from his path; that there is no barrier, except such as exists in the nature of things, between himself and whatever place in the social organization he is fitted to fill; and, more than this, that, if he has capacity and industry, a hand is held out to help him along any path which is wisely and honestly chosen.

I have endeavored to point out to you that a great deal of such an organization already exists; and I am glad to be able to add that there is a good prospect that what is wanted will, before long, be supplemented.

Those powerful and wealthy societies, the livery companies of the City of London, remembering that they are the heirs and representatives of the trade guilds of the Middle Ages, are interesting themselves in the question. So far back as 1872 the Society of Arts organized a system of instruction in the technology of arts and manufactures, for persons actually employed

in factories and workshops, who desired to extend and improve their knowledge of the theory and practice of their particular avocations; * and a considerable subsidy, in aid of the efforts of the Society, was liberally granted by the Clothworkers' Company. We have here the hopeful commencement of a rational organization for the promotion of excellence among handicraftsmen. Quite recently, other of the livery companies have determined upon giving their powerful, and, indeed, almost boundless, aid to the improvement of the teaching of handicrafts. They have already gone so far as to appoint a committee to act for them; and I betray no confidence in adding that, some time since, the committee sought the advice and assistance of several persons, myself among the number.

Of course I cannot tell you what may be the result of the deliberations of the committe; but we may all fairly hope that, before long, steps which will have a weighty and a lasting influence on the growth and spread of sound and thorough teaching among the handicraftsmen † of this country will be taken by the livery companies of London.

[This hope has been fully justified by the establishment of the Cowper Street Schools, and that of the Central Institution of the City and Guilds of London Institute.]

II.

THE CONNECTION OF THE BIOLOGICAL SCIENCES WITH MEDICINE.‡

THE great body of theoretical and practical knowledge which has been accumulated by the labors of some eighty generations, since the dawn of scientific thought in Europe, has no collective English name to which an objection may not be raised; and I use the term "medicine" as that which is least likely to be misunderstood; though, as every one knows, the name is commonly applied, in a narrower sense, to one of the chief divisions of the totality of medical science.

Taken in this broad sense, "medicine" not merely denotes a kind of knowledge, but it comprehends the various applications of that knowledge to the alleviation of the sufferings, the repair of the injuries, and the conservation of the health, of living beings. In fact, the practical aspect of medicine so far dominates over every other, that the "Healing Art" is one of its most widely-received synonyms. It is so difficult to think of medicine otherwise than as something which is necessarily connected with curative treatment, that we are apt to forget that there must be, and is, such a thing as a pure science of medicine—a "pathology" which has no more necessary subservience to practical ends than has zoology or botany.

The logical connection between this purely scientific doctrine of disease, or pathology, and ordinary biology, is easily traced. Living matter is characterized by its innate tendency to exhibit a definite series of the morphological and physiological phenomena which constitute organization and life. Given a certain range of conditions, and these phenomena remain the same, within narrow limits, for each kind of living thing. They furnish the normal and typical character of the species, and as such, they are the subject-matter of ordinary biology.

Outside the range of these conditions, the normal course of the cycle of vital phenomena is disturbed; abnormal structure makes its appearance, or the proper character and mutual adjustment of the functions cease to be preserved. The extent and the importance of these deviations

* See the "Programme" for 1878, issued by the Society of Arts, p. 14.

† It is perhaps advisable to remark that the important question of the professional education of managers of industrial works is not touched in the foregoing remarks.

‡ Address at the International Medical College, London, 1881.

from the typical life may vary indefinitely. They may have no noticeable influence on the general well-being of the economy, or they may favor it. On the other hand, they may be of such a nature as to impede the activities of the organism, or even to involve its destruction.

In the first case, these perturbations are ranged under the wide and somewhat vague category of "variations;" in the second, they are called lesions, states of poisoning, or diseases; and, as morbid states, they lie within the province of pathology. No sharp line of demarkation can be drawn between the two classes of phenomena. No one can say where anatomical variations end and tumors begin, nor where modification of function, which may at first promote health, passes into disease. All that can be said is, that whatever change of structure or function is hurtful belongs to pathology. Hence it is obvious that pathology is a branch of biology; it is the morphology, the physiology, the distribution, the ætiology of abnormal life.

However obvious this conclusion may be now, it was nowise apparent in the infancy of medicine. For it is a peculiarity of the physical sciences, that they are independent in proportion as they are imperfect; and it is only as they advance that the bonds which really unite them all become apparent. Astronomy had no manifest connection with terrestrial physics before the publication of the "Principia;" that of chemistry with physics is of still more modern revelation; that of physics and chemistry with physiology, has been stoutly denied within the recollection of most of us, and perhaps still may be.

Or, to take a case which affords a closer parallel with that of medicine. Agriculture has been cultivated from the earliest times, and, from a remote antiquity, men have attained considerable practical skill in the cultivation of the useful plants, and have empirically established many scientific truths concerning the conditions under which they flourish. But, it is within the memory of many of us, that chemistry on the one hand, and vegetable physiology on the other, attained a stage of development such that they were able to furnish a sound basis for scientific agriculture. Similarly, medicine took its rise in the practical needs of mankind. At first, studied without reference to any other branch of knowledge, it long maintained, indeed still to some extent maintains, that independence. Historically, its connection with the biological sciences has been slowly established, and the full extent and intimacy of that connection are only now beginning to be apparent. I trust I have not been mistaken in supposing that an attempt to give a brief sketch of the steps by which a philosophical necessity has become an historical reality, may not be devoid of interest, possibly of instruction, to the members of this great Congress, profoundly interested as all are in the scientific development of medicine.

The history of medicine is more complete and fuller than that of any other science, except, perhaps, astronomy; and, if we follow back the long record as far as clear evidence lights us, we find ourselves taken to the early stages of the civilization of Greece. The oldest hospitals were the temples of Æsculapius; to these Asclepeia, always erected on healthy sites, hard by fresh springs and surrounded by shady groves, the sick and the maimed resorted to seek the aid of the god of health. Votive tablets or inscriptions recorded the symptoms, no less than the gratitude, of those who were healed; and, from these primitive clinical records, the half-priestly, half-philosophic caste of the Asclepiads compiled the data upon which the earliest generalizations of medicine, as an inductive science, were based.

In this state, pathology, like all the inductive sciences at their origin, was merely natural history; it registered the phenomena of disease, classified them, and ventured upon a prognosis,

wherever the observation of constant co-existences and sequences suggested a rational expectation of the like recurrence under similar circumstances.

Further than this it hardly went. In fact, in the then state of knowledge, and in the condition of philosophical speculation at that time, neither the causes of the morbid state, nor the *rationale* of treatment, were likely to be sought for as we seek for them now. The anger of a god was a sufficient reason for the existence of a malady, and a dream ample warranty for therapeutic measures; that a physical phenomenon must needs have a physical cause was not the implied or expressed axiom that it is to us moderns.

The great man whose name is inseparably connected with the foundation of medicine, Hippocrates, certainly knew very little, indeed practically nothing, of anatomy or physiology; and he would, probably, have been perplexed, even to imagine the possibility of a connection between the zoological studies of his contemporary Democritus and medicine. Nevertheless, in so far as he, and those who worked before and after him, in the same spirit, ascertained, as matters of experience, that a wound, or a luxation, or a fever, presented such and such symptoms, and that the return of the patient to health was facilitated by such and such measures, they established laws of nature, and began the construction of the science of pathology. All true science begins with empiricism—though all true science is such exactly, in so far as it strives to pass out of the empirical stage into that of the deduction of empirical from more general truths. Thus, it is not wonderful, that the early physicians had little or nothing to do with the development of biological science; and, on the other hand, that the early biologists did not much concern themselves with medicine. There is nothing to show that the Asclepiads took any prominent share in the work of founding anatomy, physiology, zoology, and botany. Rather do these seem to have sprung from the early philosophers, who were essentially natural philosophers, animated by the characteristically Greek thirst for knowledge as such. Pythagoras, Alcmeon, Democritus, Diogenes of Apollonia, are all credited with anatomical and physiological investigations; and though Aristotle is said to have belonged to an Asclepiad family, and not improbably owed his taste for anatomical and zoological inquiries to the teachings of his father, the physician Nicomachus, the "Historia Animalium," and the treatise "De Partibus Animalium," are as free from any allusion to medicine as if they had issued from a modern biological laboratory.

It may be added, that it is not easy to see in what way it could have benefited a physician of Alexander's time to know all that Aristotle knew on these subjects. His human anatomy was too rough to avail much in diagnosis; his physiology was too erroneous to supply data for pathological reasoning. But when the Alexandrian school, with Erasistratus and Herophilus at their head, turned to account the opportunities of studying human structure, afforded to them by the Ptolemies, the value of the large amount of accurate knowledge thus obtained to the surgeon for his operations, and to the physician for his diagnosis of internal disorders, became obvious, and a connection was established between anatomy and medicine, which has ever become closer and closer. Since the revival of learning, surgery, medical diagnosis, and anatomy have gone hand in hand. Morgagni called his great work, "De sedibus et causis morborum per anatomen indagatis," and not only showed the way to search out the localities and the causes of disease by anatomy, but himself traveled wonderfully far upon the road. Bichat, discriminating the grosser constituents of the organs and parts of the body, one from another, point-

ed out the direction which modern research must take; until, at length, histology, a science of yesterday, as it seems to many of us, has carried the work of Morgagni as far as the microscope can take us, and has extended the realm of pathological anatomy to the limits of the invisible world.

Thanks to the intimate alliance of morphology with medicine, the natural history of disease has, at the present day, attained a high degree of perfection. Accurate regional anatomy has rendered practicable the exploration of the most hidden parts of the organism, and the determination, during life, of morbid changes in them; anatomical and histological post-mortem investigations have supplied physicians with a clear basis upon which to rest the classification of diseases, and with unerring tests of the accuracy or inaccuracy of their diagnoses.

If men could be satisfied with pure knowledge, the extreme precision with which, in these days, a sufferer may be told what is happening, and what is likely to happen, even in the most recondite parts of his bodily frame, should be as satisfactory to the patient as it is to the scientific pathologist who gives him the information. But I am afraid it is not; and even the practicing physician, while nowise underestimating the regulative value of accurate diagnosis, must often lament that so much of his knowledge rather prevents him from doing wrong than helps him to do right.

A scorner of physic once said that nature and disease may be compared to two men fighting, the doctor to a blind man with a club, who strikes into the *melée*, sometimes hitting the disease, and sometimes hitting nature. The matter is not mended if you suppose the blind man's hearing to be so acute that he can register every stage of the struggle, and pretty clearly predict how it will end. He had better not meddle at all, until his eyes are opened—until he can see the exact position of the antagonists, and make sure of the effect of his blows. But that which it behoves the physician to see, not, indeed, with his bodily eye, but with clear, intellectual vision, is a process, and the chain of causation involved in that process. Disease, as we have seen, is a perturbation of the normal activities of a living body, and it is, and must remain, unintelligible, so long as we are ignorant of the nature of these normal activities. In other words, there could be no real science of pathology until the science of physiology had reached a degree of perfection unattained, and indeed unattainable, until quite recent times.

So far as medicine is concerned, I am not sure that physiology, such as it was down to the time of Harvey, might as well not have existed. Nay, it is perhaps no exaggeration to say that, within the memory of living men, justly renowned practitioners of medicine and surgery knew less physiology than is now to be learned from the most elementary text-book; and, beyond a few broad facts, regarded what they did know as of extremely little practical importance. Nor am I disposed to blame them for this conclusion; physiology must be useless, or worse than useless, to pathology, so long as its fundamental conceptions are erroneous.

Harvey is often said to be the founder of modern physiology; and there can be no question that the elucidations of the function of the heart, of the nature of the pulse, and of the course of the blood, put forth in the ever-memorable little essay, "De motu cordis," directly worked a revolution in men's views of the nature and of the concatenation of some of the most important physiological processes among the higher animals; while, indirectly, their influence was perhaps even more remarkable.

But, though Harvey made this signal and perennially important contribution to the physiology of the moderns, his general conception of vital processes was essentially indentical with that of the ancients; and,

in the "Exercitationes de generatione," and notably in the singular chapter "De calido innato," he shows himself a true son of Galen and of Aristotle.

For Harvey, the blood possesses powers superior to those of the elements; it is the seat of a soul which is not only vegetative, but also sensitive and motor. The blood maintains and fashions all parts of the body, "idque summâ cum providentiâ et intellectu in finem certum agens, quasi ratiocinio quodam uteretur."

Here is the doctrine of the "pneuma," the product of the philosophical mould into which the animism of primitive men ran in Greece, in full force. Nor did its strength abate for long after Harvey's time. The same ingrained tendency of the human mind to suppose that a process is explained when it is ascribed to a power of which nothing is known except that it is the hypothetical agent of the process, gave rise, in the next century, to the animism of Stahl; and, later, to the doctrine of a vital principle, that "asylum ignorantiæ" of physiologists, which has so easily accounted for everything and explained nothing, down to our own times.

Now the essence of modern, as contrasted with ancient, physiological science appears to me to lie in its antagonism to animistic hypotheses and animistic phraseology. It offers physical explanations of vital phenomena, or frankly confesses that it has none to offer. And, so far as I know, the first person who gave expression to this modern view of physiology, who was bold enough to enunciate the proposition that vital phenomena, like all the other phenomena of the physical world, are, in ultimate analysis, resolvable into matter and motion, was René Descartes.

The fifty-four years of life of this most original and powerful thinker are widely overlapped, on both sides, by the eighty of Harvey, who survived his younger contemporary by seven years, and takes pleasure in acknowledging the French philosopher's appreciation of his great discovery.

In fact, Descartes accepted the doctrine of the circulation as propounded by "Harvæus médecin d'Angleterre," and gave a full account of it in his first work, the famous "Discours de la Méthode," which was published in 1637, only nine years after the exercitation "De motu cordis;" and, though differing from Harvey on some important points (in which it may be noted, in passing, Descartes was wrong and Harvey right), he always speaks of him with great respect. And so important does the subject seem to Descartes, that he returns to it in the "Traité des Passions," and in the "Traité de l'Homme."

It is easy to see that Harvey's work must have had a peculiar significance for the subtle thinker, to whom we owe both the spiritualistic and the materialistic philosophies of modern times. It was in the very year of its publication, 1628, that Descartes withdrew into that life of solitary investigation and meditation of which his philosophy was the fruit. And, as the course of his speculations led him to establish an absolute distinction of nature between the material and the mental worlds, he was logically compelled to seek for the explanation of the phenomena of the material world within itself; and having allotted the realm of thought to the soul, to see nothing but extension and motion in the rest of nature. Descartes uses "thought" as the equivalent of our modern term "consciousness." Thought is the function of the soul, and its only function. Our natural heat and all the movements of the body, says he, do not depend on the soul. Death does not take place from any fault of the soul, but only because some of the principal parts of the body become corrupted. The body of a living man differs from that of a dead man in the same way as a watch or other automaton (that is to say, a machine which moves of itself) when it is wound up and has,

in itself, the physical principle of the movements which the mechanism is adapted to perform, differs from the same watch, or other machine, when it is broken, and the physical principle of its movement no longer exists. All the actions which are common to us and the lower animals depend only on the conformation of our organs, and the course which the animal spirits take in the brain, the nerves, and the muscles; in the same way as the movement of a watch is produced by nothing but the force of its spring and the figure of its wheels and other parts.

Descartes' "Treatise on Man" is a sketch of human physiology, in which a bold attempt is made to explain all the phenomena of life, except those of consciousness, by physical reasonings.

To a mind turned in this direction, Harvey's exposition of the heart and vessels as a hydraulic mechanism must have been supremely welcome.

Descartes was not a mere philosophical theorist, but a hard-working dissector and experimenter, and he held the strongest opinion respecting the practical value of the new conception which he was introducing. He speaks of the importance of preserving health, and of the dependence of the mind on the body being so close that, perhaps, the only way of making men wiser and better than they are, is to be sought in medical science. "It is true," says he, "that as medicine is now practiced, it contains little that is very useful; but without any desire to depreciate, I am sure that there is no one, even among professional men, who will not declare that all we know is very little as compared with that which remains to be known; and that we might escape an infinity of diseases of the mind, no less than of the body, and even perhaps from the weakness of old age, if we had sufficient knowledge of their causes, and of all the remedies with which nature has provided us." ("Discours de la Méthode," 6e partie, Ed. Cousin, p. 193.) So strongly impressed was Descartes with this, that he resolved to spend the rest of his life in trying to acquire such a knowledge of nature as would lead to the construction of a better medical doctrine. (*Ibid.* 6e partie, Ed. Cousin, pp. 193 and 211.) The anti-Cartesians found material for cheap ridicule in these aspirations of the philosopher; and it is almost needless to say that, in the thirteen years which elapsed between the publication of the "Discours" and the death of Descartes, he did not contribute much to their realization. But, for the next century, all progress in physiology took place along the lines which Descartes laid down.

The greatest physiological and pathological work of the seventeenth century, Borelli's treatise "De Motu Animalium," is, to all intents and purposes, a development of Descartes' fundamental conception; and the same may be said of the physiology and pathology of Boerhaave, whose authority dominated in the medical world of the first half of the eighteenth century.

With the origin of modern chemistry, and of electrical science, in the latter half of the eighteenth century, aids in the analysis of the phenomena of life, of which Descartes could not have dreamed, were offered to the physiologist. And the greater part of the gigantic progress which has been made in the present century is a justification of the prevision of Descartes. For it consists, essentially, in a more and more complete resolution of the grosser organs of the living body into physico-chemical mechanisms.

"I shall try to explain our whole bodily machinery in such a way, that it will be no more necessary for us to suppose that the soul produces such movements as are not voluntary, than it is to think that there is in a clock a soul which causes it to show the hours." ("De la Formation du Fœtus.") These words of Descartes might be appropriately taken as a motto by the author of any modern treatise on physiology.

But though, as I think, there is no

doubt that Descartes was the first to propound the fundamental conception of the living body as a physical mechanism, which is the distinctive feature of modern, as contrasted with ancient physiology, he was misled by the natural temptation to carry out, in all its details, a parallel between the machines with which he was familiar, such as clocks and pieces of hydraulic apparatus, and the living machine. In all such machines there is a central source of power, and the parts of the machine are merely passive distributors of that power. The Cartesian school conceived of the living body as a machine of this kind; and herein they might have learned from Galen, who, whatever ill use he may have made of the doctrine of "natural faculties," nevertheless had the great merit of perceiving that local forces play a great part in physiology.

The same truth was recognized by Glisson, but it was first prominently brought forward in the Hallerian doctrine of the "vis insita" of muscles. If muscle can contract without nerve, there is an end of the Cartesian mechanical explanation of its contraction by the influx of animal spirits.

The discoveries of Trembley tended in the same direction. In the fresh-water *Hydra*, no trace was to be found of that complicated machinery upon which the performance of the functions in the higher animals was supposed to depend. And yet the hydra moved, fed, grew, multiplied, and its fragments, exhibited all the powers of the whole. And, finally, the work of Caspar F. Wolff, ("Theoria Generationis," 1759,) by demonstrating the fact that the growth and development of both plants and animals take place antecedently to the existence of their grosser organs, and are, in fact, the causes and not the consequences of organization (as then understood), sapped the foundations of the Cartesian physiology as a complete expression of vital phenomena.

For Wolff, the physical basis of life is a fluid, possessed of a "vis essentialis" and a "solidescibilitas," in virtue of which it gives rise to organization; and, as he points out, this conclusion strikes at the root of the whole iatro-mechanical system.

In this country, the great authority of John Hunter exerted a similar influence; though it must be admitted that the too sibylline utterances which are the outcome of Hunter's struggles to define his conceptions are often susceptible of more than one interpretation. Nevertheless, on some points Hunter is clear enough. For example, he is of opinion that "Spirit is only a property of matter" ("Introduction to Natural History," p. 6), he is prepared to renounce animism (*l. c.* p. 8), and his conception of life is so completely physical that he thinks of it as something which can exist in a state of combination in the food. "The aliment we take in has in it, in a fixed state, the real life; and this does not become active until it has got into the lungs; for there it is freed from its prison" ("Observations on Physiology," p. 113). He also thinks that "It is more in accord with the general principles of the animal machine to suppose that none of its effects are produced from any mechanical principle whatever; and that every effect is produced from an action in the part; which action is produced by a stimulus upon the part which acts, or upon some other part with which this part sympathizes so as to take up the whole action" (*l. c.* p. 152).

And Hunter is as clear as Wolff, with whose work he was probably unacquainted, that "whatever life is, it most certainly does not depend upon structure or organization" (*l. c.* p. 114).

Of course it is impossible that Hunter could have intended to deny the existence of purely mechanical operations in the animal body. But while, with Borelli and Boerhaave, he looked upon absorption, nutrition, and secretion as operations effected by means of the small vessels, he differed from the mechanical physiologists, who regarded these operations

as the result of the mechanical properties of the small vessels, such as the size, form, and disposition of their canals and apertures. Hunter, on the contrary, considers them to be the effect of properties of these vessels which are not mechanical but vital. "The vessels," says he, "have more of the polypus in them than any other part of the body," and he talks of the "living and sensitive principles of the arteries," and even of the "dispositions or feelings of the arteries." "When the blood is good and genuine the sensations of the arteries, or the dispositions for sensation, are agreeable. . . . It is then they dispose of the blood to the best advantage, increasing the growth of the whole, supplying any losses, keeping up a due succession, etc." (*l. c.* p. 133).

If we follow Hunter's conceptions to their logical issue, the life of one of the higher animals is essentially the sum of the lives of all the vessels, each of which is a sort of physiological unit, answering to a polype; and, as health is the result of the normal "action of the vessels," so is disease an effect of their abnormal action. Hunter thus stands in thought, as in time, midway between Borelli on the one hand, and Bichat on the other.

The acute founder of general anatomy, in fact, outdoes Hunter in his desire to exclude physical reasonings from the realm of life. Except in the interpretation of the action of the sense organs, he will not allow physics to have anything to do with physiology.

"To apply the physical sciences to physiology is to explain the phenomena of living bodies by the law of inert bodies. Now this is a false principle, hence all its consequences are marked with the same stamp. Let us leave to chemistry its affinity, to physics, its elasticity and its gravity. Let us invoke for physiology only sensibility and contractility." ("Anatomie générale," i. p. liv.)

Of all the unfortunate dicta of men of eminent ability this seems one of the most unhappy, when we think of what the application of the methods and the data of physics and chemistry has done toward bringing physiology into its present state. It is not too much to say that one half of a modern text-book of physiology consists of applied physics and chemistry; and that it is exactly in the exploration of the phenomena of sensibility and contractility that physics and chemistry have exerted the most potent influence.

Nevertheless, Bichat rendered a solid service to physiological progress by insisting upon the fact that what we call life, in one of the higher animals, is not an indivisible unitary archæus dominating, from its central seat, the parts of the organism, but a compound result of the synthesis of the separate lives of those parts.

"All animals," says he, "are assemblages of different organs, each of which performs its function and concurs, after its fashion, in the preservation of the whole. They are so many special machines in the general machine which constitutes the individual. But each of these special machines is itself compounded of many tissues of very different natures, which in truth constitute the elements of those organs" (*l. c.* lxxix.). "The conception of a proper vitality is applicable only to these simple tissues and not to the organs themselves" (*l. c.* lxxxiv.).

And Bichat proceeds to make the obvious application of this doctrine of synthetic life, if I may so call it, to pathology. Since diseases are only alterations of vital properties, and the properties of each tissue are distinct from those of the rest, it is evident that the diseases of each tissue must be different from those of the rest. Therefore, in any organ composed of different tissues, one may be diseased and the other remain healthy; and this is what happens in most cases (*l. c.* lxxxv.)

In a spirit of true prophecy, Bichat says, "We have arrived at an epoch, in which pathological anatomy should start afresh." For, as the analysis of

the organs had led him to the tissues, as the physiological units of the organism; so, in a succeeding generation, the analysis of the tissues led to the cell as the physiological element of the tissues. The contemporaneous study of development brought out the same result; and the zoologists and botanists, exploring the simplest and the lowest forms of animated beings, confirmed the great induction of the cell theory. Thus the apparently opposed views, which have been battling with one another ever since the middle of the last century, have proved to be each half the earth.

The proposition of Descartes that the body of a living man is a machine, the actions of which are explicable by the known laws of matter and motion, is unquestionably largely true. But it is also true, that the living body is a synthesis of innumerable physiological elements, each of which may nearly be described, in Wolff's words, as a fluid possessed of a "vis essentialis," and a "solidescibilitas"; or, in modern phrase, as protoplasm susceptible of structural metamorphosis and functional metabolism: and that the only machinery, in the precise sense in which the Cartesian school understood mechanism, is that which co-ordinates and regulates these physiological units into an organic whole.

In fact, the body is a machine of the nature of an army, not of that of a watch or of a hydraulic apparatus. Of this army each cell is a soldier, an organ, a brigade, the central nervous system headquarters and field telegraph, the alimentary and circulatory system the commissariat. Losses are made good by recruits born in camp, and the life of the individual is a campaign, conducted successfully for a number of years, but with certain defeat in the long run.

The efficacy of an army, at any given moment, depends on the health of the individual soldier, and on the perfection of the machinery by which he is led and brought into action at the proper time; and, therefore, if the analogy holds good, there can be only two kinds of diseases, the one dependent on abnormal states of the physiological units, the other on perturbations of their co-ordinating and alimentative machinery.

Hence, the establishment of the cell theory, in normal biology, was swiftly followed by a "cellular pathology," as its logical counterpart. I need not remind you how great an instrument of investigation this doctrine has proved in the hands of the man of genius to whom its development is due, and who would probably be the last to forget that abnormal conditions of the co-ordinative and distributive machinery of the body are no less important factors of disease.

Henceforward, as it appears to me, the connection of medicine with the biological sciences is clearly defined. Pure pathology is that branch of biology which defines the particular perturbation of cell-life, or of the co-ordinating machinery, or of both, on which the phenomena of disease depend.

Those who are conversant with the present state of biology will hardly hesitate to admit that the conception of the life of one of the higher animals as the summation of the lives of a cell aggregate, brought into harmonious action by a co-ordinative machinery formed by some of these cells, constitutes a permanent acquisition of physiological science. But the last form of the battle between the animistic and the physical views of life is seen in the contention whether the physical analysis of vital phenomena can be carried beyond this point or not.

There are some to whom living protoplasm is a substance, even such as Harvey conceived the blood to be, "summâ cum providentiâ et intellectu in finem certum agens, quasi ratiocinio quodam;" and who look with as little favor as Bichat did, upon any attempt to apply the principles and the methods of physics and chemistry to the investigation of the vital processes of growth, metabolism, and contractility. They stand upon the ancient ways;

only, in accordance with that progress toward democracy, which a great political writer has declared to be the fatal characteristic of modern times, they substitute a republic formed by a few billion of "animulæ" for the monarchy of the all-pervading "anima."

Others, on the contrary, supported by a robust faith in the universal applicability of the principles laid down by Descartes, and seeing that the actions called "vital" are, so far as we have any means of knowing, nothing but changes of place of particles of matter, look to molecular physics to achieve the analysis of the living protoplasm itself into a molecular mechanism. If there is any truth in the received doctrines of physics, that contrast between living and inert matter, on which Bichat lays so much stress, does not exist. In nature, nothing is at rest, nothing is amorphous; the simplest particle of that which men in their blindness are pleased to call "brute matter" is a vast aggregate of molecular mechanisms performing complicated movements of immense rapidity, and sensitively adjusting themselves to every change in the surrounding world. Living matter differs from other matter in degree and not in kind; the microcosm repeats the macrocosm; and one chain of causation connects the nebulous original of suns and planetary systems with the protoplasmic foundation of life and organization.

From this point of view, pathology is the analogue of the theory of perturbations in astronomy; and therapeutics resolves itself into the discovery of the means by which a system of forces competent to eliminate any given perturbation may be introduced into the economy. And, as pathology bases itself upon normal physiology, so therapeutics rests upon pharmacology; which is, strictly speaking, a part of the great biological topic of the influence of conditions on the living organism, and has no scientific foundation apart from physiology.

It appears to me that there is no more hopeful indication of the progress of medicine toward the ideal of Descartes than is to be derived from a comparison of the state of pharmacology, at the present day, with that which existed forty years ago. If we consider the knowledge positively acquired, in this short time, of the *modus operandi* of urari, of atropia, of physostigmin, of veratria, of casca, of strychnia, of bromide of potassium, of phosphorus, there can surely be no ground for doubting that, sooner or later, the pharmacologist will supply the physician with the means of affecting, in any desired sense, the functions of any physiological element of the body. It will, in short, become possible to introduce into the economy a molecular mechanism which, like a very cunningly-contrived torpedo, shall find its way to some particular group of living elements, and cause an explosion among them, leaving the rest untouched.

The search for the explanation of diseased states in modified cell-life; the discovery of the important part played by parasitic organisms in the ætiology of disease; the elucidation of the action of medicaments by the methods and the data of experimental physiology; appear to me to be the greatest steps which have ever been made toward the establishment of medicine on a scientific basis. I need hardly say they could not have been made except for the advance of normal biology.

There can be no question, then, as to the nature or the value of the connection between medicine and the biological sciences. There can be no doubt that the future of pathology and of therapeutics, and, therefore, that of practical medicine, depends upon the extent to which those who occupy themselves with these subjects are trained in the methods and impregnated with the fundamental truths of biology.

And, in conclusion, I venture to suggest that the collective sagacity of this Congress could occupy itself with

no more important question than with this: How is medical education to be arranged, so that, without entangling the student in those details of the systematist which are valueless to him, he may be enabled to obtain a firm grasp of the great truths respecting animal and vegetable life, without which, notwithstanding all the progress of scientific medicine, he will still find himself an empiric?

III.

JOSEPH PRIESTLEY.

If the man to perpetuate whose memory we have this day raised a statue had been asked on what part of his busy life's work he set the highest value, he would undoubtedly have pointed to his voluminous contributions to theology. In season and out of season, he was the steadfast champion of that hypothesis respecting the Divine nature which is termed Unitarianism by its friends and Socinianism by its foes. Regardless of odds, he was ready to do battle with all comers in that cause; and if no adversaries entered the lists, he would sally forth to seek them.

To this, his highest ideal of duty, Joseph Priestley sacrificed the vulgar prizes of life, which, assuredly, were within easy reach of a man of his singular energy and varied abilities. For this object, he put aside, as of secondary importance, those scientific investigations which he loved so well, and in which he showed himself so competent to enlarge the boundaries of natural knowledge and to win fame. In this cause, he not only cheerfully suffered obloquy from the bigoted and the unthinking, and came within sight of martyrdom; but bore with that which is much harder to be borne than all these, the unfeigned astonishment and hardly disguised contempt of a brilliant society, composed of men whose sympathy and esteem must have been most dear to him, and to whom it was simply incomprehensible that a philosopher should seriously occupy himself with any form of Christianity.

It appears to me that the man who, setting before himself such an ideal of life, acted up to it consistently, is worthy of the deepest respect, whatever opinion may be entertained as to the real value of the tenets which he so zealously propagated and defended.

But I am sure that I speak not only for myself, but for all this assemblage, when I say that our purpose to-day is to do honor, not to Priestley, the Unitarian divine, but to Priestley, the fearless defender of rational freedom in thought and in action; to Priestley, the philosophic thinker; to that Priestley who held a foremost place among "the swift runners who hand over the lamp of life," and transmit from one generation to another the fire kindled, in the childhood of the world, at the Promethean altar of Science.

The main incidents of Priestley's life are so well known that I need dwell upon them at no great length.

Born in 1733, at Fieldhead, near Leeds, and brought up among Calvinists of the straitest orthodoxy, the boy's striking natural ability led to his being devoted to the profession of a minister of religion; and, in 1752, he was sent to the Dissenting Academy at Daventry—an institution which authority left undisturbed, though its existence contravened the law. The teachers under whose instruction and influence the young man came at Daventry, carried out to the letter the injunction to "try all things: hold fast that which is good," and encouraged the discussion of every imaginable proposition with complete freedom, the leading professors taking opposite sides; a discipline which, admirable as it may be from a purely scientific point of view, would seem to be calculated to make acute, rather than sound, divines. Priestley tells us, in his "Autobiography," that he generally found himself on the unorthodox side: and as he grew older, and his faculties at-

tained their maturity, this native tendency toward heterodoxy grew with his growth and strengthened with his strength. He passed from Calvinism to Arianism; and finally, in middle life, landed in that very broad form of Unitarianism, by which his craving after a credible and consistent theory of things was satisfied.

On leaving Daventry, Priestley became minister of a congregation, first at Needham Market, and secondly at Nantwich; but whether on account of his heterodox opinions, or of the stuttering which impeded his expression of them in the pulpit, little success attended his efforts in this capacity. In 1761, a career much more suited to his abilities became open to him. He was appointed "tutor in the languages" in the Dissenting Academy at Warrington, in which capacity, besides giving three courses of lectures, he taught Latin, Greek, French, and Italian, and read lectures on the Theory of Language and Universal Grammar, on Oratory, Philosophical Criticism, and Civil law. And it is interesting to observe that, as a teacher, he encouraged and cherished in those whom he instructed, the freedom which he had enjoyed, in his own student days, at Daventry. One of his pupils tells us that,

"At the conclusion of his lecture, he always encouraged his students to express their sentiments relative to the subject of it, and to urge any objections to what he had delivered, without reserve. It pleased him when any one commenced such a conversation. In order to excite the freest discussion, he occasionally invited the students to drink tea with him, in order to canvass the subjects of his lectures. I do not recollect that he ever showed the least displeasure at the strongest objections that were made to what he delivered, but I distinctly remember the smile of approbation with which he usually received them; nor did he fail to point out, in a very encouraging manner, the ingenuity or force of any remarks that were made, when they merited these characters. His object, as well as Dr. Aikin's, was to engage the students to examine and decide for themselves, uninfluenced by the sentiments of any other persons." ("Life and Correspondence of Dr. Priestley," by J. T. Rutt. Vol. i. p. 50.)

It would be difficult to give a better description of a model teacher than that conveyed in these words.

From his earliest days, Priestley had shown a strong bent toward the study of nature; and his brother Timothy tells us that the boy put spiders into bottles to see how long they would live in the same air—a curious anticipation of the investigations of his later years. At Nantwich, where he set up a school, Priestley informs us that he bought an air pump, an electrical machine, and other instruments, in the use of which he instructed his scholars. But he does not seem to have devoted himself seriously to physical science until 1766, when he had the great good fortune to meet Benjamin Franklin, whose friendship he ever afterward enjoyed. Encouraged by Franklin, he wrote a "History of Electricity," which was published in 1767, and appears to have met with considerable success.

In the same year, Priestley left Warrington to become the minister of a congregation at Leeds; and, here, happening to live next door to a public brewery, as he says,

"I, at first, amused myself with making experiments on the fixed air which I found ready-made in the process of fermentation. When I removed from that house I was under the necessity of making fixed air for myself; and one experiment leading to another, as I have distinctly and faithfully noted in my various publications on the subject, I by degrees contrived a convenient apparatus for the purpose, but of the cheapest kind.

"When I began these experiments I knew very little of *chemistry*, and had, in a manner, no idea on the subject before I attended a course of chemical lectures, delivered in the Academy at Warrington, by Dr. Turner of Liverpool. But I have often thought that, upon the whole, this circumstance was no disadvantage to me; as, in this situation, I was led to devise an apparatus and processes of my own, adapted to my peculiar views; whereas, if I had been previously accustomed to the usual chemical processes, I should not have so easily thought of any other, and without new modes of operation, I should hardly have discovered anything materially new." ("Autobiography," §§ 100, 101.)

The first outcome of Priestley's chemical work, published in 1772, was of a very practical character. He dis-

covered the way of impregnating water with an excess of "fixed air," or carbonic acid, and thereby producing what we now know as "soda water"—a service to naturally, and still more to artificially, thirsty souls which those whose parched throats and hot heads are cooled by morning draughts of that beverage, cannot too greatfully acknowledge. In the same year, Priestley communicated the extensive series of observations which his industry and ingenuity had accumulated, in the course of four years, to the Royal Society, under the title of "Observations on Different Kinds of Air"—a memoir which was justly regarded of so much merit and importance, that the Society at once conferred upon the author the highest distinction in their power, by awarding him the Copley Medal.

In 1771 a proposal was made to Priestley to accompany Captain Cook in his second voyage to the South Seas. He accepted it, and his congregation agreed to pay an assistant to supply his place during his absence. But the appointment lay in the hands of the Board of Longitude, of which certain clergymen were members; and whether these worthy ecclesiastics feared that Priestley's presence among the ship's company might expose his Majesty's Sloop *Resolution* to the fate which aforetime befell a certain ship that went from Joppa to Tarshish; or whether they were alarmed lest a Socinian should undermine that piety which, in the days of Commodore Trunnion, so strikingly characterized sailors, does not appear, but, at any rate, they objected to Priestley "on account of his religious principles," and appointed the two Forsters, whose "religious principles," if they had been known to these well-meaning but not far-sighted persons, would probably have surprised them.

In 1772 another proposal was made to Priestley. Lord Shelburne, desiring a "literary companion," had been brought into communication with Priestley by the good offices of a friend of both, Dr. Price; and offered him the nominal post of librarian, with a good house and appointments, and an annuity in case of the termination of the engagement. Priestley accepted the offer, and remained with Lord Shelburne for seven years, sometimes residing at Calne, sometimes travelling abroad with the Earl.

Why the connection terminated has never been exactly known; but it is certain that Lord Shelburne behaved with the utmost consideration and kindness toward Priestley, that he fulfilled his engagements to the letter; and that, at a later period, he expressed a desire that Priestley should return to his old footing in his house. Probably enough, the politician, aspiring to the highest offices in the state, may have found the position of the protector of a man who was being denounced all over the country as an infidel and an atheist somewhat embarrassing. In fact, a passage in Priestley's "Autobiography" on the occasion of the publication of his "Disquisitions relating to Matter and Spirit," which took place in 1777, indicates pretty clearly the state of the case:—

"(126) It being probable that this publication would be unpopular, and might be the means of bringing odium on my patron, several attempts were made by his friends, though none by himself, to dissuade me from persisting in it. But being, as I thought, engaged in the cause of important truth, I proceeded without regard to any consequences, assuring them that this publication should not be injurious to his lordship."

It is not unreasonable to suppose that his lordship, as a keen, practical man of the world, did not derive much satisfaction from this assurance. The "evident marks of dissatisfaction" which Priestley says he first perceived in his patron in 1778, may well have arisen from the peer's not unnatural uneasiness as to what his domesticated, but not tamed, philosopher might write next, and what storm might thereby be brought down on his own head; and it speaks very highly for Lord Shelburne's delicacy that, in the midst of such perplexities, he

made not the least attempt to interfere with Priestley's freedom of action. In 1780, however, he intimated to Dr. Price that he should be glad to establish Priestley on his Irish estates: the suggestion was interpreted, as Lord Shelburne probably intended it should be, and Priestley left him, the annuity of £150 a year, which had been promised in view of such a contingency, being punctually paid.

After leaving Calne, Priestley spent some little time in London, and then, having settled in Birmingham at the desire of his brother-in-law, he was soon invited to become the minister of a large congregation. This settlement Priestley considered, at the time, to be "the happiest event of his life." And well he might think so; for it gave him competence and leisure; placed him within reach of the best makers of apparatus of the day; made him a member of that remarkable "Lunar Society," at whose meetings he could exchange thoughts with such men as Watt, Wedgewood, Darwin, and Boulton; and threw open to him the pleasant house of the Galtons of Barr, where these men, and others of less note, formed a society of exceptional charm and intelligence.*

But these halcyon days were ended by a bitter storm. The French Revolution broke out. An electric shock ran through the nations; whatever there was of corrupt and retrograde, and at the same time, a great deal of what there was of best and noblest, in European society shuddered at the outburst of long-pent-up social fires. Men's feelings were excited in a way that we, in this generation, can hardly comprehend. Party wrath and virulence were expressed in a manner unparalleled, and it is to be hoped impossible, in our times; and Priestley and his friends were held up to public scorn, even in Parliament, as fomenters of sedition. A "Church-and-King" cry was raised against the Liberal Dissenters; and, in Birmingham, it was intensified and specially directed toward Priestley by a local controversy, in which he had engaged with his usual vigor. In 1791, the celebration of the second anniversary of the taking of the Bastille by a public dinner, with which Priestley had nothing whatever to do, gave the signal to the loyal and pious mob, who, unchecked, and indeed to some extent encouraged, by those who were responsible for order, had the town at their mercy for three days. The chapels and houses of the leading Dissenters were wrecked, and Priestley and his family had to fly for their lives, leaving library, apparatus, papers, and all their possessions, a prey to the flames.

Priestley never returned to Birmingham. He bore the outrages and losses inflicted upon him with extreme patience and sweetness,* and betook himself to London. But even his scientific colleagues gave him a cold shoulder; and though he was elected minister of a congregation at Hackney, he felt his position to be insecure, and finally determined on emigrating to the United States. He

* See "The Life of Mary Anne Schimmelpenninck." Mrs. Schimmelpenninck (*née* Galton) remembered Priestley very well, and her description of him is worth quotation :—"A man of admirable simplicity, gentleness and kindness of heart, united with great acuteness of intellect. I can never forget the impression produced on me by the serene expression of his countenance. He, indeed, seemed present with God by recollection, and with man by cheerfulness. I remember that, in the assembly of these distinguished men, among whom Mr. Boulton, by his noble manner, his fine countenance (which much resembled that of Louis XIV.), and princely munificence, stood pre-eminently as the great Mecænas; even as a child, I used to feel, when Dr. Priestley entered after him, that the glory of the one was terrestrial, that of the other celestial; and utterly far as I am removed from a belief in the sufficiency of Dr. Priestley's theological creed, I cannot but here record this evidence of the eternal power of any portion of the truth held in its vitality."

* Even Mrs. Priestley, who might be forgiven for regarding the destroyers of her household gods with some asperity, contents herself, in writing to Mrs. Barbauld, with the sarcasm that the Birmingham people "will scarcely find so many respectable characters, a second time, to make a bonfire of."

landed in America in 1794; lived quietly with his sons at Northumberland, in Pennsylvania, where his posterity still flourished; and, clear-headed and busy to the last, died on the 6th of February, 1804.

Such were the conditions under which Joseph Priestley did the work which lay before him, and then, as the Norse Sagas say, went out of the story. The work itself was of the most varied kind. No human interest was without its attraction for Priestley, and few men have ever had so many irons in the fire at once; but, though he may have burned his fingers a little, very few who have tried that operation have burned their fingers so little. He made admirable discoveries in science; his philosophical treatises are still well worth reading; his political works are full of insight and replete with the spirit of freedom; and while all these sparks flew off from his anvil, the controversial hammer rained a hail of blows on orthodox priest and bishop. While thus engaged, the kindly, cheerful doctor felt no more wrath or uncharitableness toward his opponents than a smith does toward his iron. But if the iron could only speak!—and the priests and bishops took the point of view of the iron.

No doubt what Priestley's friends repeatedly urged upon him—that he would have escaped the heavier trials of his life and done more for the advancement of knowledge, if he had confined himself to his scientific pursuits and let his fellow-men go their way—was true. But it seems to have been Priestley's feeling that he was a man and a citizen before he was a philosopher, and that the duties of the two former positions are at least as imperative as those of the latter. Moreover, there are men (and I think Priestley was one of them) to whom the satisfaction of throwing down a triumphant fallacy is as great as that which attends the discovery of a new truth; who feel better satisfied with the government of the world, when they have been helping Providence by knocking an imposture on the head; and who care even more for freedom of thought than for mere advance of knowledge. These men are the Carnots who organize victory for truth, and they are, at least, as important as the generals who visibly fight her battles in the field.

Priestley's reputation as a man of science rests upon his numerous and important contributions to the chemistry of gaseous bodies; and to form a just estimate of the value of his work—of the extent to which it advanced the knowledge of fact and the development of sound theoretical views—we must reflect what chemistry was in the first half of the eighteenth century.

The vast science which now passes under that name had no existence. Air, water, and fire were still counted among the elemental bodies; and though Van Helmont, a century before, had distinguished different kinds of air as *gas ventosum* and *gas sylvestre*, and Boyle and Hales had experimentally defined the physical properties of air, and discriminated some of the various kinds of aëriform bodies, no one suspected the existence of the numerous totally distinct gaseous elements which are now known, or dreamed that the air we breathe and the water we drink are compounds of gaseous elements.

But, in 1754, a young Scotch physician, Dr. Blank, made the first clearing in this tangled backwood of knowledge. And it gives one a wonderful impression of the juvenility of scientific chemistry to think that Lord Brougham, whom so many of us recollect, attended Black's lectures when he was a student in Edinburgh. Black's researches gave the world the novel and startling conception of a gas that was a permanently elastic fluid like air, but that differed from common air in being much heavier, very poisonous, and in having the properties of an acid, capable of neutralizing the strongest alkalies; and it took the world some time to become accustomed to the notion.

A dozen years later, one of the most sagacious and accurate investigators who has adorned this, or any other, country, Henry Cavendish, published a memoir in the "Philosophical Transactions," in which he deals not only with the "fixed air" (now called carbonic acid or carbonic anhydride) of Black, but with "inflammable air," or what we now term hydrogen.

By the rigorous application of weight and measure to all his processes, Cavendish implied the belief subsequently formulated by Lavoisier, that, in chemical processes, matter is neither created nor destroyed, and indicated the path along which all future explorers must travel. Nor did he himself halt until this path led him, in 1784, to the brilliant and fundamental discovery that water is composed of two gases united in fixed and constant proportions.

It is a trying ordeal for any man to be compared with Black and Cavendish, and Priestley cannot be said to stand on their level. Nevertheless, his achievements are not only great in themselves, but truly wonderful, if we consider the disadvantages under which he labored. Without the careful scientific training of Black, without the leisure and appliances secured by the wealth of Cavendish, he scaled the walls of science as so many Englishmen have done before and since his day; and trusting to mother wit to supply the place of training, and to ingenuity to create apparatus out of washing tubs, he discovered more new gases than all his predecessors put together had done. He laid the foundations of gas analysis; he discovered the complementary actions of animal and vegetable life upon the constituents of the atmosphere; and, finally, he crowned his work, this day one hundred years ago, by the discovery of that "pure dephlogisticated air" to which the French chemists subsequently gave the name of oxygen. Its importance, as the constituent of the atmosphere which disappears in the processes of respiration and combustion, and is restored by green plants growing in sunshine, was proved somewhat later. For these brilliant discoveries, the Royal Society elected Priestley a fellow and gave him their medal, while the Academies of Paris and St. Petersburg conferred their membership upon him. Edinburgh had made him an honorary doctor of laws at an early period of his career; but, I need hardly add, that a man of Priestley's opinions received no recognition from the universities of his own country.

That Priestley's contributions to the knowledge of chemical fact were of the greatest importance, and that they richly deserve all the praise that has been awarded to them, is unquestionable; but it must, at the same time, be admitted that he had no comprehension of the deeper significance of his work; and, so far from contributing anything to the theory of the facts which he discovered, or assisting in their rational explanation, his influence to the end of his life was warmly exerted in favor of error. From first to last, he was a stiff adherent of the phlogiston doctrine which was prevalent when his studies commenced; and, by a curious irony of fate, the man who by the discovery of what he called "dephlogisticated air" furnished the essential datum for the true theory of combustion, of respiration, and of the composition of water, to the end of his days fought against the inevitable corollaries from his own labors. His last scientific work, published in 1800, bears the title, "The Doctrine of Phlogiston established, and that of the Composition of Water refuted."

When Priestley commenced his studies, the current belief was, that atmospheric air, freed from accidental impurities, is a simple elementary substance, indestructible and unalterable, as water was supposed to be. When a combustible burned, or when an animal breathed in air, it was supposed that a substance, "phlogiston," the matter of heat and light, passed from the burning or breathing body into it,

and destroyed its powers of supporting life and combustion. Thus, air contained in a vessel in which a lighted candle had gone out, or a living animal had breathed until it could breath no longer, was called "phlogisticated." The same result was supposed to be brought about by the addition of what Priestley called "nitrous gas" to common air.

In the course of his researches, Priestley found that the quantity of common air which can thus become "phlogisticated," amounts to about one-fifth the volume of the whole quantity submitted to experiment. Hence it appeared that common air consists, to the extent of four-fifths of its volume, of air which is already "phlogisticated;" while the other fifth is free from phlogiston, or "dephlogisticated." On the other hand, Priestley found that air "phlogisticated" by combustion or respiration could be "dephlogisticated," or have the properties of pure common air restored to it, by the action of green plants in sunshine. The question, therefore, would naturally arise—as common air can be wholly phlogisticated by combustion, and converted into a substance which will no longer support combustion, is it possible to get air that shall be less phlogisticated than common air, and consequently support combustion better than common air does?

Now, Priestley says that, in 1774, the possibility of obtaining air less phlogisticated than common air had not occurred to him. ("Experiments and Observations on Different Kinds of Air, vol. ii. p. 31.) But in pursuing his experiments on the evolution of air from various bodies by means of heat, it happened that, on the 1st of August, 1774, he threw the heat of the sun, by means of a large burning glass which he had recently obtained, upon a substance which was then called *mercurius calcinatus per se*, and which is commonly known as red precipitate.

"I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can well express, was that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air, exposed to iron or lime of sulphur; but as I had got nothing like this remarkable appearance from any kind of air besides this particular modification of nitrous air, and I knew no nitrous acid was used in the preparation of *mercurius calcinatus* I was utterly at a loss how to account for it.

"In this case also, though I did not give sufficient attention to the circumstance at that time, the flame of the candle, besides being larger, burned with more splendor and heat than in that species of nitrous air; and a piece of red-hot wood sparkled in it, exactly like paper dipped in a solution of nitre, and it consumed very fast—an experiment which I had never thought of trying with nitrous air." (*Ibid.* pp. 34, 35).

Priestley obtained the same sort of air from red lead, but, as he says himself, he remained in ignorance of the properties of this new kind of air for seven months, or until March, 1775, when he found that the new air behaved with "nitrous gas" in the same way as the dephlogisticated part of common air does; * but that, instead of being diminished to four-fifths, it almost completely vanished, and, therefore, showed itself to be "between five and six times as good as the best common air I have ever met with." (*Ibid.* p. 48.) As this new air thus appeared to be completely free from phlogiston, Priestley called it "dephlogisticated air."

What was the nature of this air? Priestley found that the same kind of air was to be obtained by moistening with the spirit of niter (which he terms nitrous acid) any kind of earth that is free from phlogiston, and applying heat; and consequently he says: "There remained no doubt on my mind but that the atmospherical air, or the thing that we breathe, consists of the nitrous acid and earth, with so much pholgiston as is necessary to its elasticity, and likewise so much more as is required to bring it from

* "Experiments and Observations on Different Kinds of Air," vol. ii. p. 40.

its state of perfect purity to the mean condition in which we find it." (*Ibid.* p. 55.)

Priestley's view, in fact, is that atmospheric air is a kind of saltpetre, in which the potash is replaced by some unknown earth. And in speculating on the manner in which saltpetre is formed, he enunciates the hypothesis "that niter is formed by a real *decomposition of the air itself*, the *bases* that are presented to it having, in such circumstances, a nearer affinity with the spirit of niter than that kind of earth with which it is united in the atmosphere." (*Ibid.* p. 60. The italics are Priestley's own.)

It would have heen hard for the most ingenious person to have wandered farther from the truth than Priestley does in this hypothesis; and, though Lavoisier undoubtedly treated Priestley very ill, and pretended to have discovered dephlogisticated air, or oxygen, as he called it, independently, we can almost forgive him, when we reflect how different were the ideas which the great French chemist attached to the body which Priestley discovered.

They are like two navigators of whom the first sees a new country, but takes clouds for mountains and mirage for lowlands; while the second determines its length and breadth, and lays down on a chart its exact place, so that, thenceforth, it serves as a guide to his successors, and becomes a secure outpost whence new explorations may be pushed.

Nevertheless, as Priestley himself somewhere remarks, the first object of physical science is to ascertain facts, and the service which he rendered to chemistry by the definite establishment of a large number of new and fundamentally important facts, is such as to entitle him to a very high place among the fathers of chemical science.

It is difficult to say whether Priestley's philosophical, political, or theological views were most responsible for the bitter hatred which was borne to him by a large body of his countrymen,* and which found its expression in the malignant insinuations in which Burke, to his everlasting shame, indulged in the House of Commons.

Without containing much that will be new to the readers of Hobbes, Spinoza, Collins, Hume, and Hartley, and, indeed, while making no pretensions to originality, Priestley's "Disquisitions relating to Matter and Spirit," and his "Doctrine of Philosophical Necessity illustrated," are among the most powerful, clear, and unflinching expositions of materialism and necessarianism which exist in the English language, and are still well worth reading.

Priestley denied the freedom of the will in the sense of its self-determination; he denied the existence of a soul distinct from the body; and as a natural consequence, he denied the natural immortality of man.

In relation to these matters English opinion, a century ago, was very much what it is now.

A man may be a necessarian without incurring graver reproach than that implied in being called a gloomy fanatic, necessarianism, though very shocking, having a note of Calvinistic orthodoxy; but, if a man is a materialist; or, if good authorities say he is and must be so, in spite of his assertion to the contrary; or, if he acknowl-

*"In all the newspapers and most of the periodical publications I was represented as an unbeliever in Revelation, and no better than an atheist."—"Autobiography," Rutt. vol. i. p. 124. "On the walls of houses, etc., and especially where I usually went, were to be seen, in large characters, 'MADAN FOREVER; DAMN PRIESTLEY; NO PRESBYTERIANISM; DAMN THE PRESBYTERIANS,' etc., etc.; and, at one time, I was followed by a number of boys, who left their play, repeating what they had seen on the walls, and shouting out, '*Damn Priestley; damn him, damn him, forever, forever*,' etc., etc. This was no doubt a lesson which they had been taught by their parents, and what they, I fear, had learned from their superiors."—"Appeal to the Public on the Subject of the Riots at Birmingham."

edge himself unable to see good reasons for believing in the natural immortality of man, respectable folks look upon him as an unsafe neighbor of a cash-box, as an actual or potential sensualist, the more virtuous in outward seeming, the more certainly loaded with secret "grave personal sins."

Nevertheless, it is as certain as anything can be, that Joseph Priestley was no gloomy fanatic, but as cheerful and kindly a soul as ever breathed, the idol of children; a man who was hated only by those who did not know him, and who charmed away the bitterest prejudices in personal intercourse; a man who never lost a friend, and the best testimony to whose worth is the generous and tender warmth with which his many friends vied with one another in rendering him substantial help, in all the crises of his career.

The unspotted purity of Priestley's life, the strictness of his performance of every duty, his transparent sincerity, the unostentatious and deep-seated piety which breathes through all his correspondence, are in themselves a sufficient refutation of the hypothesis invented by bigots to cover uncharitableness, that such opinions as his must arise from moral defects. And his statue will do as good service as the brazen image that was set upon a pole before the Israelites, if those who have been bitten by the fiery serpents of sectarian hatred, which still haunt this wilderness of a world, are made whole by looking upon the image of a heretic, who was yet a saint.

Though Priestley did not believe in the natural immortality of man, he held with an almost naïve realism, that man would be raised from the dead by a direct exertion of the power of God, and thenceforward be immortal. And it may be as well for those who may be shocked by this doctrine to know that views, substantially identical with Priestley's, have been advocated, since his time, by two prelates of the Anglican Church: by Dr. Whately, Archbishop of Dublin, in his well-known "Essays;"* and by Dr. Courtenay, Bishop of Kingston in Jamaica, the first edition of whose remarkable book "On the Future States," dedicated to Archbishop Whately, was published in 1843 and the second in 1857. According to Bishop Courtenay,

> "The death of the body will cause a cessation of all the activity of the mind by way of natural consequence; to continue forever UNLESS the Creator should interfere."

And again:—

> "The natural end of human existence is the 'first death,' the dreamless slumber of the grave, wherein man lies spellbound, soul and body, under the dominion of sin and death—that whatever modes of conscious existence, whatever future states of 'life' or of 'torment' beyond Hades are reserved for man, are results of our blessed Lord's victory over sin and death; that the resurrection of the dead must be preliminary to their entrance into either of the future states, and that the nature and even existence of these states and even the mere fact that there is a futurity of consciousness, can be known *only* through God's revelation of Himself in the Person and the Gospel of His Son."—P. 389.

And now hear Priestley:—

> "Man, according to this system (of materialism), is no more than we now see of him. His being commences at the time of his conception, or perhaps at an earlier period. The corporeal and mental faculties, in being in the same substance, grow, ripen, and decay together; and whenever the system is dissolved it continues in a state of dissolution till it shall please that Almighty Being who called it into existence to restore it to life again."—"Matter and Spirit," p. 49.

And again:—

> "The doctrine of the Scripture is, that God made man of the dust of the ground, and by simply animating this organized matter, made man that living percipient and intelligent being that he is. According to Revelation, *death* is a state of rest and insensibility, and our only though sure hope of a future life is founded on the doctrine of the resurrection of the whole man at some distant period; this assurance being sufficently confirmed to us both by the evident tokens of a Divine

* First series. "On Some of the Peculiarities of the Christian Religion." Essay I. Revelation of a Future State.

commission attending the persons who delivered the doctrine, and especially by the actual resurrection of Jesus Christ, which is more authentically attested than any other fact in history."—*Ibid.*, p. 247.

We all know that "a saint in crape is twice a saint in lawn;" but it is not yet admitted that the views which are consistent with such saintliness in lawn, become diabolical when held by a mere dissenter.*

I am not here either to defend or to attack Priestley's philosophical views, and I cannot say that I am personally disposed to attach much value to episcopal authority in philosophical questions; but it seems right to call attention to the fact, that those of Priestley's opinions which have brought most odium upon him, have been openly promulgated, without challenge, by persons occupying the highest positions in the State Church.

I must confess that what interests me most about Priestley's materialism, is the evidence that he saw dimly the seed of destruction which such materialism carries within its own bosom. In the course of his reading for his "History of Discoveries relating to Vision, Light, and Colors," he had come upon the speculations of Boscovich and Michell and had been led to admit the sufficiently obvious truth that our knowledge of matter is a knowledge of its properties; and that of its substance—if it have a substance—we know nothing. And this led to the further admission that, so far as we can know, there may be no difference between the substance of matter and the substance of spirit ("Disquisitions," p. 16). A step farther would have shown Priestley that his materialism was, essentially, very little different from the Idealism of his contemporary, the Bishop of Cloyne.

As Priestley's philosophy is mainly a clear statement of the views of the deeper thinkers of his day, so are his political conceptions based upon those of Locke. Locke's aphorism that "the end of government is the good of mankind," is thus expanded by Priestley:—

"It must necessarily be understood, therefore, whether it be expressed or not, that all people live in society for their mutual advantage; so that the good and happiness of the members, that is, of the majority of the members, of any state, is the great standard by which everything relating to that state must finally be determined." ("Essay on the First Principles of Government." Second edition, 1771, p. 13.)

The little sentence here interpolated, "that is, of the majority of the members of any state," appears to be that passage which suggested to Bentham, according to his own acknowledgment, the famous "greatest happiness" formula, which by substituting "happiness" for "good," has converted a noble into an ignoble principle. But I do not call to mind that there is any utterance in Locke quite so outspoken as the following passage in the "Essay on the First Principles of Government." After laying down as "a fundamental maxim in all governments," the proposition that "kings, senators, and nobles" are "the servants of the public," Priestley goes on to say:—

"But in the largest states, if the abuses of the government should at any time be great and manifest; if the servants of the people, forgetting their masters and their masters' interest, should pursue a separate one of their own; if, instead of considering that they are made for the people, they should consider the people as made for them; if the oppressions and violation of right should be great, flagrant, and universally resented; if

* Not only is Priestley at one with Bishop Courtenay in this matter, but with Hartley and Bonnet, both of them stout champions of Christianity. Moreover, Archbishop Whately's essay is little better than an expansion of the first paragraph of Hume's famous essay on the Immortality of the Soul:—"By the mere light of reason it seems difficult to prove the immortality of the soul; the arguments for it are commonly derived either from metaphysical topics, or moral, or physical. But it is in reality the Gospel, and the Gospel alone, that has brought *life and immortality to light*." It is impossible to imagine that a man of Whately's tastes and acquirements had not read Hume or Hartley, though he refers to neither.

the tyrannical governors should have no friends but a few sycophants, who had long preyed upon the vitals of their fellow-citizens, and who might be expected to desert a government whenever their interests should be detached from it; if in consequence of these circumstances, it should become manifest that the risk which would be run in attempting a revolution would be trifling, and the evils which might be apprehended from it were far less than those which were actually suffered and which were daily increasing; in the name of God, I ask, what principles are those which ought to restrain an injured and insulted people from asserting their natural rights, and from changing or even punishing their governors—that is, their servants—who had abused their trust, or from altering the whole form of their government, if it appeared to be of a structure so liable to abuse?"

As a Dissenter, subject to the operation of the Corporation and Test Acts, and as a Unitarian, excluded from the benefit of the Toleration Act, it is not surprising to find that Priestley had very definite opinions about Ecclesiastical Establishments; the only wonder is that these opinions were so moderate as the following passages show them to have been:—

"Ecclesiastical authority may have been necessary in the infant state of society, and, for the same reason, it may perhaps continue to be, in some degree, necessary as long as society is imperfect; and therefore may not be entirely abolished till civil governments have arrived at a much greater degree of perfection. If, therefore, I were asked whether I should approve of the immediate dissolution of all the ecclesiastical establishments in Europe, I should answer, No. . . Let experiment be first made of *alterations*, or, which is the same thing, of *better establishments* than the present. Let them be reformed in many essential articles, and then not thrown aside entirely till it be found by experience that no good can be made of them."

Priestley goes on to suggest four such reforms of a capital nature:—

"1. Let the Articles of Faith to be subscribed by candidates for the ministry be greatly reduced. In the formulary of the Church of England, might not thirty-eight out of the thirty-nine be very well spared? It is a reproach to any Christian establishment if every man cannot claim the benefit of it who can say that he believes in the religion of Jesus Christ as it is set forth in the New Testament. You say the terms are so general that even Deists would quibble and insinuate themselves. I answer that all the articles which are subscribed at present, by no means exclude Deists who will prevaricate; and upon this scheme you would at least exclude fewer honest men."*

The second reform suggested is the equalization, in proportion to work done, of the stipends of the clergy; the third, the exclusion of the bishops from Parliament; and the fourth, complete toleration, so that every man may enjoy the rights of a citizen, and be qualified to serve his country, whether he belong to the Established Church or not.

Opinions such as those I have quoted, respecting the duties and the responsibilities of governors, are the commonplaces of modern Liberalism; and Priestley's views on Ecclesiastical Establishments would, I fear, meet with but a cool reception, as altogether too conservative, from a large proportion of the lineal descendants of the people who taught their children to cry "Damn Priestley;" and, with that love for the practical application of science which is the source of the greatness of Birmingham, tried to set fire to the doctor's house with sparks from his own electrical machine; thereby giving the man they called an incendiary and raiser of sedition against Church and King, an appropriately experimental illustration of the nature of arson and riot.

If I have succeeded in putting before you the main features of Priestley's work, its value will become apparent, when we compare the condition of the English nation, as he knew it, with its present state.

The fact that France has been for eighty-five years trying, without much success, to right herself after the great storm of the Revolution, is not unfrequently cited among us, as an indication of some inherent incapacity for self-government among the French people. I think, however, that Englishmen who argue thus, forget that, from the meeting of the

* "Utility of Establishments," in "Essay on First Principles of Government," p. 198, 1771.

Long Parliament in 1640, to the last Stuart rebellion in 1745, is a hundred and five years, and that, in the middle of the last century, we had but just safely freed ourselves from our Bourbons and all that they represented. The corruption of our state was as bad as that of the Second Empire. Bribery was the instrument of government, and peculation its reward. Four-fifths of the seats in the House of Commons were more or less openly dealt with as property. A minister had to consider the state of the vote market, and the sovereign secured a sufficiency of "king's friends" by payments allotted with retail, rather than royal, sagacity.

Barefaced and brutal immorality and intemperance pervaded the land, from the highest to the lowest classes of society. The Established Church was torpid, so far as it was not a scandal; but those who dissented from it came within the meshes of the Act of Uniformity, the Test Act, and the Corporation Act. By law, such a man as Priestley, being a Unitarian, could neither teach nor preach, and was liable to ruinous fines and long imprisonment.* In those days, the guns that were pointed by the Church against the Dissenters were shotted. The law was a cesspool of iniquity and cruelty. Adam Smith was a new prophet whom few regarded, and commerce was hampered by idiotic impediments, and ruined by still more absurd help, on the part of government.

Birmingham, though already the center of a considerable industry, was a mere village as compared with its present extent. People who traveled went about armed, by reason of the abundance of highwaymen and the paucity and inefficiency of the police. Stage coaches had not reached Birmingham, and it took three days to get to London. Even canals were a recent and much opposed invention.

* In 1732 Doddridge was cited for teaching without the Bishop's leave, at Northampton.

Newton had laid the foundation of a mechanical conception of the physical universe: Hartley, putting a modern face upon ancient materialism, had extended that mechanical conception to psychology; Linnæus and Haller were beginning to introduce method and order into the chaotic accumulation of biological facts. But those parts of physical science which deal with heat, electricity, and magnetism, and above all, chemistry, in the modern sense, can hardly be said to have had an existence. No one knew that two of the old elemental bodies, air and water, are compounds, and that a third, fire, is not a substance but a motion. The great industries that have grown out of the applications of modern scientific discoveries had no existence, and the man who should have foretold their coming into being in the days of his son, would have been regarded as a mad enthusiast.

In common with many other excellent persons, Priestley believed that man is capable of reaching, and will eventually attain, perfection. If the temperature of space presented no obstacle, I should be glad to entertain the same idea; but judging from the past progress of our species, I am afraid that the globe will have cooled down so far, before the advent of this natural millennium, that we shall be, at best, perfected Esquimaux. For all practical purposes, however, it is enough that man may visibly improve his condition in the course of a century or so. And, if the picture of the state of things in Priestley's time, which I have just drawn, have any pretense to accuracy, I think it must be admitted that there has been a considerable change for the better.

I need not advert to the well-worn topic of material advancement, in a place in which the very stones testify to that progress—in the town of Watt and of Boulton. I will only remark, in passing, that material advancement has its share in moral and intellectual progress. Becky Sharp's acute remark that it is not difficult to be virtuous on ten thousand a year, has its

application to nations; and it is futile to expect a hungry and squalid population to be anything but violent and gross. But as regards other than material welfare, although perfection is not yet in sight—even from the masthead—it is surely true that things are much better than they were.

Take the upper and middle classes as a whole, and it may be said that open immorality and gross intemperance have vanished. Four and six bottle men are as extinct as the dodo. Women of good repute do not gamble, and talk modeled upon Dean Swift's "Art of Polite Conversation" would be tolerated in no decent kitchen.

Members of the legislature are not to be bought; and constituents are awakening to the fact that votes must not be sold—even for such trifles as rabbits and tea and cake. Political power has passed into the hands of the masses of the people. Those whom Priestley calls their servants have recognized their position, and have requested the master to be so good as to go to school and fit himself for the administration of his property. No civil disability attaches to any one on theological grounds, and the highest offices of the state are open to Papist, Jew, or Secularist.

Whatever men's opinions as to the policy of Establishment, no one can hesitate to admit that the clergy of the Church are men of pure life and conversation, zealous in the discharge of their duties; and, at present, apparently, more bent on prosecuting one another than on meddling with Dissenters. Theology itself has broadened so much, that Anglican divines put forward doctrines more liberal than those of Priestley; and, in our state-supported churches, one listener may hear a sermon to which Bossuet might have given his approbation, while another may hear a discourse in which Socrates would find nothing new.

But great as these changes may be, they sink into insignificance beside the progress of physical science, whether we consider the improvement of methods of investigation, or the increase in bulk of solid knowledge. Consider that the labors of Laplace, of Young, of Davy, and of Faraday; of Cuvier, of Lamarck, and of Robert Brown; of Von Baer, and of Schwann; of Smith and of Hutton, have all been carried on since Priestley discovered oxygen; and consider that they are now things of the past, concealed by the industry of those who have built upon them, as the first founders of a coral reef are hidden beneath the life's work of their successors; consider that the methods of physical science are slowly spreading into all investigations, and that proofs as valid as those required by her canons of investigation, are being demanded of all doctrines which ask for men's assent; and you will have a faint image of the astounding difference in this respect between the nineteenth century and the eighteenth.

If we ask what is the deeper meaning of all these vast changes, I think there can be but one reply. They mean that reason has asserted and exercised her primacy over all provinces of human activity: that ecclesiastical authority has been relegated to its proper place; that the good of the governed has been finally recognized as the end of government, and the complete responsibility of governors to the people as its means; and that the dependence of natural phenomena in general, on the laws of action of what we call matter has become an axiom.

But it was to bring these things about, and to enforce the recognition of these truths, that Joseph Priestley labored. If the nineteenth century is other and better than the eighteenth, it is, in great measure, to him and to such men as he, that we owe the change. If the twentieth century is to be better than the nineteenth, it will be because there are among us men who walk in Priestley's footsteps.

Such men are not those whom their own generation delights to honor; such men, in fact, rarely trouble themselves about honor, but ask, in

another spirit than Falstaff's, "What is honor? Who hath it? He that died o' Wednesday." But whether Priestley's lot be theirs, and a future generation, in justice and in gratitude, set up their statues; or whether their names and fame are blotted out from remembrance, their work will live as long as time endures. To all eternity, the sun of truth and right will have been increased by their means; to all eternity, falsehood and injustice will be the weaker because they have lived.

IV.

ON SENSATION AND THE UNITY OF STRUCTURE OF SENSIFEROUS ORGANS.*

THE maxim that metaphysical inquiries are barren of result, and that the serious occupation of the mind with them is a mere waste of time and labor, finds much favor in the eyes of the many persons who pride themselves on the possession of sound common sense; and we sometimes hear it enunciated by weighty authorities, as if its natural consequence, the suppression of such studies, had the force of a moral obligation.

In this case, however, as in some others, those who lay down the law seem to forget that a wise legislator will consider, not merely whether his proposed enactment is desirable, but whether obedience to it is possible. For, if the latter question is answered negatively, the former is surely hardly worth debate.

Here, in fact, lies the pith of the reply to those who would make metaphysics contraband of intellect. Whether it is desirable to place a prohibitory duty upon philosophical speculations or not, it is utterly impossible to prevent the importation of them into the mind. And it is not a little curious to observe that those who most loudly profess to abstain from such commodities are, all the while, unconscious consumers, on a great scale, of one or other of their multitudinous disguises or adulterations. With mouths full of the particular kind of heavily buttered toast which they affect, they inveigh against the eating of plain bread. In truth, the attempt to nourish the human intellect upon a diet which contains no metaphysics is about as hopeful as that of certain Eastern sages to nourish their bodies without destroying life. Everybody has heard the story of the pitiless microscopist, who ruined the peace of mind of one of these mild enthusiasts by showing him the animals moving in a drop of the water with which, in the innocency of his heart, he slaked his thirst; and the unsuspecting devotee of plain common sense may look for as unexpected a shock when the magnifier of severe logic reveals the germs, if not the full-grown shapes, of lively metaphysical postulates rampant amidst his most positive and matter-of-fact notions.

By way of escape from the metaphysical Will-o'-the-wisps generated in the marshes of literature and theology, the serious student is sometimes bidden to betake himself to the solid ground of physical science. But the fish of immortal memory, who threw himself out of the frying-pan into the fire, was not more ill advised than the man who seeks sanctuary from philosophical persecution within the walls of the observatory or of the laboratory. It is said that "metaphysics" owe their name to the fact that, in Aristotle's works, questions of pure philosophy are dealt with immediately after those of physics. If so, the accident is happily symbolical of the essential relations of things; for metaphysical speculation follows as closely upon physical theory as black care upon the horseman.

One need but mention such fundamental, and indeed indispensable, conceptions of the natural philosopher

* Address at the Royal Institution, London, 1880.

as those of atoms and forces: or that of attraction considered as action at a distance: or that of potential energy; or the antinomies of a vacuum and a plenum; to call to mind the metaphysical background of physics and chemistry; while, in the biological sciences, the case is still worse. What is an individual among the lower plants and animals? Are genera and species realities or abstractions? Is there such a thing as Vital Force? or does the name denote a mere relic of metaphysical fetichism? Is the doctrine of final causes legitimate or illegitimate? These are a few of the metaphysical topics which are suggested by the most elementary study of biological facts. But, more than this, it may be truly said that the roots of every system of philosophy lie deep among the facts of physiology. No one can doubt that the organs and the functions of sensation are as much a part of the province of the physiologist, as are the organs and functions of motion, or those of digestion; and yet it is impossible to gain an acquaintance with even the rudiments of the physiology of sensation without being led straight to one of the most fundamental of all metaphysical problems. In fact, the sensory operations have been, from time immemorial, the battle-ground of philosophers.

I have more than once taken occasion to point out that we are indebted to Descartes, who happened to be a physiologist as well as a philosopher, for the first distinct enunciation of the essential elements of the true theory of sensation. In later times, it is not to the works of the philosophers, if Hartley and James Mill are excepted, but to those of the physiologists, that we must turn for an adequate account of the sensory process. Haller's luminous, though summary, account of sensation in his admirable "Primæ Lineæ," the first edition of which was printed in 1747, offers a striking contrast to the prolixity and confusion of thought which pervade Reid's "Inquiry," of seventeen years' later date.* Even Sir William Hamilton, learned historian and acute critic as he was, not only failed to apprehend the philosophical bearing of long-established physiological truths; but, when he affirmed that there is no reason to deny that the mind feels at the finger points, and none to assert that the brain is the sole organ of thought, he showed that he had not apprehended the significance of the revolution commenced, two hundred years before his time, by Descartes, and effectively followed up by Haller, Hartley, and Bonnet, in the middle of the last century.†

In truth, the theory of sensation, except in one point, is, at the present moment, very much where Hartley, led by a hint of Sir Isaac Newton's, left it, when, a hundred and twenty years since, the "Observations on Man: his Frame, his Duty, and his Expectations," was laid before the world. The whole matter is put in a nutshell in the following passages of this notable book.

"External objects impressed upon the senses occasion, first on the nerves on which they are impressed, and then on the brain, vibrations of the small and, as we may say, infinitesimal medullary particles.

"These vibrations are motions backward and forward of the small particles; of the same kind with the oscillations of pendulums and the tremblings of the particles of sounding bodies. They must be conceived to be exceedingly short and small, so as not to have the least efficacy to disturb or move the whole bodies of the nerves or brain." (Vol. 1, p. 11.)

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* In justice to Reid, however, it should be stated that the chapters on sensation in the "Essays on the Intellectual Powers" (1785) exhibit a great improvement. He is, in fact, in advance of his commentator, as the note to Essay II. chap. ii. p. 248 of Hamilton's edition shows.

† Sir William Hamilton gravely informs his hearers:—"We have no more right to deny that the mind feels at the finger points, as consciousness assures us, than to assert that it thinks exclusively in the brain."—"Lecture on Metaphysics and Logic," ii. p. 128. "We have no reason whatever to doubt the report of consciousness, that we actually perceive at the external point of sensation, and that we perceive the material reality."—*Ibid.* p. 129.

"The white medullary substance of the brain is also the immediate instrument by which ideas are presented to the mind; or, in other words, whatever changes are made in this substance, corresponding changes are made in our ideas; and *vice versa*." (*Ibid.* p. 8.)*

Hartley, like Haller, had no conception of the nature and functions of the gray matter of the brain. But, if for "white medullary substance," in the latter paragraph, we substitute "gray cellular substance," Hartley's propositions embody the most probable conclusions which are to be drawn from the latest investigations of physiologists. In order to judge how completely this is the case, it will be well to study some simple case of sensation, and, following the example of Reid and of James Mill, we may begin with the sense of smell. Suppose that I become aware of a musky scent, to which the name of "muskiness" may be given. I call this an odor, and I class it along with the feelings of light, colors, sounds, tastes, and the like, among those phenomena which are known as sensations. To say that I am aware of this phenomenon, or that I have it, or that it exists, are simply different modes of affirming the same facts. If I am asked how I know that it exists, I can only reply that its existence and my knowledge of it are one and the same thing; in short, that my knowledge is immediate or intuitive, and, as such, is possessed of the highest conceivable degree of certainty.

The pure sensation of muskiness is almost sure to be followed by a mental state which is not a sensation, but a belief, that there is somewhere close at hand a something on which the existence of the sensation depends. It may be a musk-deer, or a musk-rat, or a musk-plant, or a grain of dry musk, or simply a scented handkerchief; but former experience leads us to believe that the sensation is due to the presence of one or other of these objects, and that it will vanish if the object is removed. In other words, there arises a belief in an external cause of the muskiness, which, in common language, is termed an odorous body.

But the manner in which this belief is usually put into words is strangely misleading. If we are dealing with a musk-plant, for example, we do not confine ourselves to a simple statement of that which we believe, and say that the musk-plant is the cause of the sensation called muskiness; but we say that the plant has a musky smell, and we speak of the odor as a quality, or property, inherent in the plant. And the inevitable reaction of words upon thought has in this case become so complete, and has penetrated so deeply, that when an accurate statement of the case—namely, that muskiness, inasmuch as the term denotes nothing but a sensation, is a mental state, and has no existence except as a mental phenomenon—is first brought under the notice of common-sense folks, it is usually regarded by them as what they are pleased to call a mere metaphysical paradox and a patent example of useless subtlety. Yet the slightest reflection must suffice to convince any one possessed of sound reasoning faculties, that it is as absurd to suppose that muskiness is a quality inherent in one plant, as it would be to imagine that pain is a quality inherent in another because we feel pain when a thorn pricks the finger.

Even the common-sense philosopher, *par excellence*, says of smell: "It appears to be a simple and original affection or feeling of the mind, altogether inexplicable and unaccountable. It is indeed impossible that it can be in anybody: it is a sensation, and a sensation can only be in a sentient thing." *

* The speculations of Bonnet are remarkably similar to those of Hartley; and they appear to have originated independently, though the "Essai de Psychologie" (1754) is of five years' later date than the "Observations on Man" (1749).

* "An Inquiry into the Human Mind on the Principles of Common Sense," chap. ii. § 2. Reid affirms that "it is genius, and no

That which is true of muskiness is true of every other odor. Lavender-smell, clove-smell, garlic-smell, are, like "muskiness," names of states of consciousness, and have no existence except as such. But, in ordinary language, we speak of all these odors as if they were independent entities residing in lavender, cloves, and garlic; and it is not without a certain struggle that the false metaphysic of so-called common sense, thus ingrained in us, is expelled.

For the present purpose, it is unnecessary to inquire into the origin of our belief in external bodies, or into that of the notion of causation. Assuming the existence of an external world, there is no difficulty in obtaining experimental proof that, as a general rule, olfactory sensations are caused by odorous bodies; and we may pass on to the next step of the inquiry—namely, how the odorous body produces the effect attributed to it.

The first point to be noted here is another fact revealed by experience; that the appearance of the sensation is governed, not only by the presence of the odorous substance, but by the condition of a certain part of our corporeal structure, the nose. If the nostrils are closed, the presence of the odorous substance does not give rise to the sensation; while, when they are open, the sensation is intensified by the approximation of the odorous substance to them, and by snuffing up the adjacent air in such a manner as to draw it into the nose. On the other hand, looking at an odorous substance, or rubbing it on the skin, or holding it to the ear, does not awaken the sensation. Thus, it can be readily established by experiment that the perviousness of the nasal passages is, in some way, essential to the sensory function; in fact, that the organ of that function is lodged somewhere in the nasal passages. And, since odorous bodies give rise to their effects at considerable distances, the suggestion is obvious that something must pass from them into the sense organ. What is this "something," which plays the part of an intermediary between the odorous body and the sensory organ?

The oldest speculation about the matter dates back to Democritus and the Epicurean School, and it is to be found fully stated in the fourth book of Lucretius. It comes to this: that the surfaces of bodies are constantly throwing off excessively attenuated films of their own substance: and that these films, reaching the mind, excite the appropriate sensations in it.

Aristotle did not admit the existence of any such material films, but conceived that it was the form of the substance, and not its matter, which affected sense, as a seal impresses wax, without losing anything in the process. While many, if not the majority, of the Schoolmen took up an intermediate position and supposed that a something, which was not exactly either material or immaterial, and which they called an "intentional species," effected the needful communication between the bodily cause of sensation and the mind.

But all these notions, whatever may be said for or against them in general, are fundamentally defective, by reason of an oversight which was inevitable, in the state of knowledge at the time in which they were promulgated. What the older philosophers did not know, and could not know, before the anatomist and the physiologist had done their work, is that, between the external object and that mind in which they supposed the sensation to inhere, there lies a physical obstacle. The sense organ is not a mere passage by which the "tenuia simulacra rerum," or the "intentional

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the want of it, that adulterates philosophy, and fills it with error and false theory;" and no doubt his own lucubrations are free from the smallest taint of the impurity to which he objects. But, for want of something more than that sort of "common sense," which is very common and a little dull, the contemner of genius did not notice that the admission here made knocks so big a hole in the bottom of "common sense philosophy," that nothing can save it from foundering in the dreaded abyss of Idealism.

species" cast off by objects, or the "forms" of sensible things, pass straight to the mind; on the contrary, it stands as a firm and impervious barrier, through which no material particle of the world without can make its way to the world within.

Let us consider the olfactory sense organ more nearly. Each of the nostrils leads into a passage completely separated from the other by a partition, and these two passages place the nostrils in free communication with the back of the throat, so that they freely transmit the air passing to the lungs when the mouth is shut, as in ordinary breathing. The floor of each passage is flat, but its roof is a high arch, the crown of which is seated between the orbital cavities of the skull, which serve for the lodgment and protection of the eyes; and it therefore lies behind the apparent limits of that feature which, in ordinary language, is called the nose. From the side walls of the upper and back part of these arched chambers, certain delicate plates of bone project, and these, as well as a considerable part of the partition between the two chambers, are covered by a fine, soft, moist membrane. It is to this "Schneiderian," or olfactory, membrane that odorous bodies must obtain direct access, if they are to give rise to their appropriate sensations; and it is upon the relatively large surface, which the olfactory membrane offers, that we must seek for the seat of the organ of the olfactory sense. The only essential part of that organ consists of a multitude of minute rod-like bodies, set perpendicularly to the surface of the membrane, and forming a part of the cellular coat, or epithelium, which covers the olfactory membrane, as the epidermis covers the skin. In the case of the olfactory sense, there can be no doubt that the Democritic hypothesis, at any rate for such odorous substances as musk, has a good foundation. Infinitesimal particles of musk fly off the surface of the odorous body, and, becoming diffused through the air, are carried into the nasal passages, and thence into the olfactory chambers, where they come into contact with the filamentous extremities of the delicate olfactory epithelium.

But this is not all. The "mind" is not, so to speak, upon the other side of the epithelium. On the contrary, the inner ends of the olfactory cells are connected with nerve fibers, and these nerve fibers, passing into the cavity of the skull, at length end in a part of the brain, the olfactory sensorium. It is certain that the integrity of each, and the physical inter-connection of all these three structures, the epithelium of the sensory organ, the nerve fibers, and the sensorium, are essential conditions of ordinary sensation. That is to say, the air in the olfactory chambers may be charged with particles of musk; but, if either the epithelium, or the nerve fibers, or the sensorium is injured, or if they are physically disconnected from one another, sensation will not arise. Moreover, the epithelium may be said to be receptive, the nerve fibers transmissive, and the sensorium sensifacient. For, in the act of smelling, the particles of the odorous substance produce a molecular change (which Hartley was in all probability right in terming a vibration) in the epithelium, and this change being transmitted to the nerve fibers, passes along them with a measurable velocity, and, finally reaching the sensorium, is immediately followed by the sensation.

Thus, modern investigation supplies a representative of the Epicurean simulacra in the volatile particles of the musk; but it also gives us the stamp of the particles on the olfactory epithelium, without any transmission of matter, as the equivalent of the Aristotelian "form;" while, finally, the modes of motion of the molecules of the olfactory cells, of the nerve, and of the cerebral sensorium, which are Hartley's vibrations, may stand very well for a double of the "intentional species" of the Schoolmen. And this last remark is not intended

merely to suggest a fanciful parallel; for, if the cause of the sensation is, as analogy suggests, to be sought in the mode of motion of the object of sense, then it is quite possible that the particular mode of motion of the object is reproduced in the sensorium; exactly as the diaphragm of a telephone reproduces the mode of motion taken up at its receiving end. In other words, the secondary "intentional species" may be, as the Schoolmen thought the primary one was, the last link between matter and mind.

None the less, however, does it remain true that no similarity exists, nor indeed is conceivable, between the cause of the sensation and the sensation. Attend as closely to the sensations of muskiness, or any other odor, as we will, no trace of extension, resistance, or motion is discernible in them. They have no attribute in common with those which we ascribe to matter; they are, in the strictest sense of the words, immaterial entities.

Thus, the most elementary study of sensation justifies Descartes' position, that we know more of mind than we do of body; that the immaterial world is a firmer reality than the material. For the sensation "muskiness" is known immediately. So long as it persists, it is a part of what we call our thinking selves, and its existence lies beyond the possibility of doubt. The knowledge of an objective or material cause of the sensation, on the other hand, is mediate; it is a belief as contradistinguished from an intuition; and it is a belief which, in any given instance of sensation, may by possibility, be devoid of foundation. For odors, like other sensations, may arise from the occurrence of the appropriate molecular changes in the nerve or in the sensorium, by the operation of a cause distinct from the affection of the sense organ by an odorous body. Such "subjective" sensations are as real existences as any others, and as distinctly suggest an external odorous object as their cause; but the belief thus generated is a delusion. And, if beliefs are properly termed "testimonies of consciousness," then undoubtedly the testimony of consciousness may be, and often is, untrustworthy.

Another very important consideration arises out of the facts as they are now known. That which, in the absence of a knowledge of the physiology of sensation, we call the cause of the smell, and term the odorous object, is only such, mediately, by reason of its emitting particles which give rise to a mode of motion in the sense organ. The sense organ, again, is only a mediate cause by reason of its producing a molecular change in the nerve fiber; while this last change is also only a mediate cause of sensation, depending, as it does, upon the change which it excites in the sensorium.

The sense organ, the nerve, and the sensorium, taken together, constitute the sensiferous apparatus. They make up the thickness of the wall between the mind, as represented by the sensation "muskiness" and the object, as represented by the particle of musk in contact with the olfactory epithelium.

It will be observed that the sensiferous wall and the external world are of the same nature; whatever it is that constitutes them both is expressible in terms of matter and motion. Whatever changes take place in the sensiferous apparatus are continuous with, and similar to, those which take place in the external world.* But with the sensorium, matter and motion come to an end; while phenomena of another order, or immaterial states of consciousness, make their appearance. How is the relation between the material and the immaterial phenomena to be conceived? This is the metaphysical problem of problems, and the solutions which have been suggested have been made the corner-stones of systems of philosophy. Three mutually

* See note on the next page.

irreconcilable readings of the riddle have been offered.

The first is, that an immaterial substance of mind exists; and that it is affected by the mode of motion of the sensorium in such a way as to give rise to the sensation

The second is, that the sensation is a direct effect of the mode of motion of the sensorium, brought about without the intervention of any substance of mind.

The third is, that the sensation is neither directly nor indirectly an effect of the mode of motion of the sensorium, but that it has an independent cause. Properly speaking, therefore, it is not an effect of the motion of the sensorium, but a concomitant of it.

As none of these hypotheses is capable of even an approximation to demonstration, it is almost needless to remark that they have been severally held with tenacity and advocated with passion. I do not think it can be said of any of the three that it is inconceivable, or that it can be assumed on *a priori* grounds to be impossible.

Consider the first, for example; an immaterial substance is perfectly conceivable. In fact, it is obvious that if we possessed no sensations but those of smell and hearing, we should be unable to conceive a material substance. We might have a conception of time, but could have none of extension, or of resistance or of motion. And without the three latter conceptions no idea of matter could be formed. Our whole knowledge would be limited to that of a shifting succession of immaterial phenomena. But, if an immaterial substance may exist, it may have any conceivable properties; and sensation may be one of them. All these propositions may be affirmed with complete dialectic safety, inasmuch as they cannot possibly be disproved; but neither can a particle of demonstrative evidence be offered in favor of the existence of an immaterial substance.

As regards the second hypothesis, it certainly is not inconceivable, and therefore it may be true, that sensation is the direct effect of certain kinds of bodily motion. It is just as easy to suppose this as to suppose, on the former hypothesis, that bodily motion affects an immaterial substance. But neither is it susceptible of proof.

And, as to the third hypothesis, since the logic of induction is in no case competent to prove that events apparently standing in the relation of

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Note.—The following diagrammatic scheme may help to elucidate the theory of sensation:

Mediate Knowledge				Immediate Knowledge.
	Sensiferous Apparatus			
Objects of Sense	Receptive (Sense Organ)	Transmissive (Nerve)	Sensificatory (Sensorium)	Sensations and other States of Consciousness
Hypothetical Substance of Matter.				Hypothetical Substance of Mind
Physical World				Mental World
Not Self	Self			
Non-Ego or Object				Ego or Subject

Immediate knowledge is confined to states of consciousness, or, in other words, to the phenomena of mind. Knowledge of the physical world, or of one's own body and of objects external to it, is a system of beliefs or judgments based on the sensations. The term "self" is applied not only to the series of mental phenomena which constitute the ego, but to the fragment of the physical world which is their constant concomitant. The corporeal self, therefore, is part of the non-ego; and is objective in relation to the ego as subject.

cause and effect may not both be effects of a common cause—that also is as safe from refutation, if as incapable of demonstration, as the other two.

In my own opinion, neither of these speculations can be regarded seriously as anything but a more or less convenient working hypothesis. But, if I must choose among them, I take the "law of parcimony" for my guide, and select the simplest—namely, that the sensation is the direct effect of the mode of motion of the sensorium. It may justly be said that this is not the slightest explanation of sensation; but then am I really any the wiser, if I say that a sensation is an activity (of which I know nothing) of a substance of mind (of which also I know nothing)? Or, if I say that the Deity causes the sensation to arise in my mind immediately after He has caused the particles of the sensorium to move in a certain way, is anything gained? In truth a sensation, as we have already seen, is an intuition—a part of immediate knowledge. As such, it is an ultimate fact and inexplicable; and all that we can hope to find out about it, and that indeed is worth finding out, is its relation to other natural facts. That relation appears to me to be sufficiently expressed, for all practical purposes, by saying that sensation is the invariable consequent of certain changes in the sensorium—or, in other words, that, so far as we know, the change in the sensorium is the cause of the sensation.

I permit myself to imagine that the untutored, if noble, savage of "common sense" who has been misled into reading thus far by the hope of getting positive solid information about sensation, giving way to not unnatural irritation, may here interpellate: "The upshot of all this long disquisition is that we are profoundly ignorant. We knew that to begin with, and you have merely furnished another example of the emptiness and uselessness of metaphysics." But I venture to reply, Pardon me, you were ignorant, but you did not know it. On the contrary, you thought you knew a great deal, and were quite satisfied with the particularly absurd metaphysical notions which you were pleased to call the teachings of common sense. You thought that your sensations were properties of external things, and had an existence outside of yourself. You thought that you knew more about material than you do about immaterial existences. And if, as a wise man has assured us, the knowledge of what we don't know is the next best thing to the knowledge of what we do know, this brief excursion into the province of philosophy has been highly profitable.

Of all the dangerous mental habits, that which schoolboys call "cocksureness" is probably the most perilous; and the inestimable value of metaphysical discipline is that it furnishes an effectual counterpoise to this evil proclivity. Whoso has mastered the elements of philosophy knows that the attribute of unquestionable certainty appertains only to the existence of a state of consciousness so long as it exists; all other beliefs are mere probabilities of a higher or lower order. Sound metaphysic is an amulet which renders its professor proof alike against the poison of superstition and the counterpoison of nihilism; by showing that the affirmations of the former and the denials of the latter alike deal with matters about which, for lack of evidence, nothing can be either affirmed or denied.

I have dwelt at length upon the nature and origin of our sensations of smell, on account of the comparative freedom of the olfactory sense from the complications which are met with in most of the other senses.

Sensations of taste, however, are generated in almost as simple a fashion as those of smell. In this case, the sense organ is the epithelium which covers the tongue and the palate: and which sometimes, becoming modified, gives rise to peculiar organs termed "gustatory bulbs,"

in which the epithelial cells elongate and assume a somewhat rod-like form. Nerve fibers connect the sensory organ with the sensorium, and tastes or flavors are states of consciousness caused by the change of molecular state of the latter. In the case of the sense of touch there is often no sense organ distinct from the general epidermis. But many fishes and amphibia exhibit local modifications of the epidermic cells which are sometimes extraordinarily like the gustatory bulbs; more commonly, both in lower and higher animals, the effect of the contact of external bodies is intensified by the development of hair-like filaments, or of true hairs, the bases of which are in immediate relation with the ends of the sensory nerves. Every one must have noticed the extreme delicacy of the sensations produced by the contact of bodies with the ends of the hairs of the head; and the "whiskers" of cats owe their functional importance to the abundant supply of nerves to the follicles in which their bases are lodged. What part, if any, the so-called "tactile corpuscles," "end bulbs," and "Pacinian bodies," play in the mechanism of touch is unknown. If they are sense organs, they are exceptional in character, in so far as they do not appear to be modifications of the epidermis. Nothing is known respecting the organs of those sensations of resistance which are grouped under the head of the muscular sense; nor of the sensations of warmth and cold; nor of that very singular sensation which we call tickling.

In the case of heat and cold, the organism not only becomes affected by external bodies, far more remote than those which affect the sense of smell; but the Democritic hypothesis is obviously no longer permissible. When the direct rays of the sun fall upon the skin, the sensation of heat is certainly not caused by "attenuated films" thrown off from that luminary, but is due to a mode of motion which is transmitted to us. In Aristotelian phrase, it is the form without the matter of the sun which stamps the sense organ; and this, translated into modern language, means nearly the same thing as Hartley's vibrations. Thus we are prepared for what happens in the case of the auditory and the visual senses. For neither the ear, nor the eye, receives anything but the impulses or vibrations originated by sonorous or luminous bodies. Nevertheless, the receptive apparatus still consists of nothing but specially modified epithelial cells. In the labyrinth of the ear of the higher animals, the free ends of these cells terminate in excessively delicate hair-like filaments; while, in the lower forms of auditory organ, its free surface is beset with delicate hairs like those of the surface of the body, and the transmissive nerves are connected with the bases of these hairs. Thus there is an insensible gradation in the forms of the receptive apparatus from the organ of touch, on the one hand, to those of taste and smell; and, on the other hand, to that of hearing. Even in the case of the most refined of all the sense organs, that of vision, the receptive apparatus departs but little from the general type. The only essential constituent of the visual sense organ is the retina, which forms so small a part of the eyes of the higher animals; and the simplest eyes are nothing but portions of the integument, in which the cells of the epidermis have become converted into glassy, rod-like retinal corpuscles. The outer ends of these are turned toward the light; their sides are more or less extensively coated with a dark pigment, and their inner ends are connected with the transmissive nerve fibers. The light, impinging on these visual rods, produces a change in them which is communicated to the nerve fibers, and, being transmitted to the sensorium, gives rise to the sensation—if indeed all animals which possess eyes are endowed with what we understand as sensation.

In the higher animals, a complicated appartus of lenses, arranged on the principle of a camera obscura, serves at once to concentrate and to individualize the pencils of light proceeding from external bodies. But the essential part of the organ of vision is still a layer of cells, which have the form of rods with truncated or conical ends. By what seems a strange anomaly, however, the glassy ends of these are turned not toward, but away from, the light; and the latter has to traverse the layer of nervous tissues with which their outer ends are connected, before it can affect them. Moreover, the rods and cones of the vertebrate retina are so deeply seated, and in many respects so peculiar in character, that it appears impossible, at first sight, that they can have any thing to do with that epidermis of which gustatory and tactile, and at any rate the lower forms of auditory and visual, organs are obvious modifications.

Whatever be the apparent diversities among the sensiferous apparatuses, however, they share certain common characters. Each consists of a receptive, a transmissive, and a sensificatory portion. The essential part of the first is an epithelium, of the second, nerve fibers, of the third, a part of the brain; the sensation is always the consequence of the mode of motion excited in the receptive, and sent along the transmissive, to the sensificatory part of the sensiferous apparatus. And, in all the senses, there is no likeness whatever between the object of sense, which is matter in motion, and the sensation, which is an immaterial phenomena.

On the hypothesis which appears to me to be the most convenient, sensation is a product of the sensiferous apparatus caused by certain modes of motion which are set up in it by impulses from without. The sensiferous apparatuses are, as it were, factories, all of which at the one end receive raw materials of a similar kind—namely, modes of motion—while, at the other, each turns out a special product, the feeling which constitutes the kind of sensation characteristic of it.

Or, to make use of a closer comparison, each sensiferous apparatus is comparable to a musical-box wound up; with as many tunes as there are separate sensations. The object of a simple sensation is the agent which presses down the stop of one of these tunes, and the more feeble the agent, the more delicate must be the mobility of the stop.

But if this be true, if the recipient part of the sensiferous apparatus is, in all cases, merely a mechanism affected by coarser or finer kinds of material motion, we might expect to find that all sense organs are fundamentally alike, and result from the modification of the same morphological elements. And this is exactly what does result from all recent histological and embryological investigations.

It has been seen that the receptive part of the olfactory apparatus is a slightly modified epithelium, which lines an olfactory chamber deeply seated between the orbits in adult human beings. But, if we trace back the nasal chambers to their origin in the embryo, we find, that, to begin with, they are mere depressions of the skin of the fore part of the head, lined by a continuation of the general epidermis. These depressions become pits, and the pits, by the growth of the adjacent parts, gradually acquire the position which they finally occupy. The olfactory organ, therefore, is a specially modified part of the general integument.

The human ear would seem to present greater difficulties. For the essential part of the sense organ, in this case, is the membranous labyrinth, a bag of complicated form, which lies buried in the depths of the floor of the skull, and is surrounded by dense and solid bone. Here, however, recourse to the study of development readily unravels the mystery. Shortly after the time when the olfactory organ appears, as a depression of the skin

on the side of the fore part of the head, the auditory organ appears, as a similar depression on the side of its back part. The depression, rapidly deepening, becomes a small pouch; and then, the communication with the exterior becoming shut off, the pouch is converted into a closed bag, the epithelial lining of which is a part of the general epidermis segregated from the rest. The adjacent tissues, changing first into cartilage and then into bone, enclose the auditory sac in a strong case, in which it undergoes its further metamorphoses; while the drum, the ear bones, and the external ear, are superadded by no less extraordinary modifications of the adjacent parts. Still more marvelous is the history of the development of the organ of vision. In the place of the eye, as in that of the nose and that of the ear, the young embryo presents a depression of the general integument; but, in man and the higher animals, this does not give rise to the proper sensory organ, but only to part of the accessory structures concerned in vision. In fact, this depression, deepening and becoming converted into a shut sac, produces only the cornea, the aqueous humor, and the crystalline lens of the perfect eye.

The retina is added to this by the outgrowth of the wall of a portion of the brain into a sort of bag, or sac, with a narrow neck, the convex bottom of which is turned outward, or toward the crystalline lens. As the development of the eye proceeds, the convex bottom of the bag becomes pushed in, so that it gradually obliterates the cavity of the sac, the previously convex wall of which becomes deeply concave. The sac of the brain is now like a double nightcap ready for the head, but the place which the head would occupy is taken by the vitreous humor, while the layer of nightcap next it becomes the retina. The cells of this layer which lie farthest from the vitreous humor, or, in other words, bound the original cavity of the sac, are metamorphosed into the rods and cones. Suppose now that the sac of the brain could be brought back to its original form; then the rods and cones would form part of the lining of a side pouch of the brain. But one of the most wonderful revelations of embryology is the proof of the fact that the brain itself is, at its first beginning, merely an infolding of the epidermic layer of the general integument. Hence it follows that the rods and cones of the vertebrate eye are modified epidermic cells, as much as the crystalline cones of the insect or crustacean eye are; and that the inversion of the position of the former in relation to light arises simply from the roundabout way in which the vertebrate retina is developed.

Thus all the higher sense organs start from one foundation, and the receptive epithelium of the eye, or of the ear, is as much modified epidermis as is that of the nose. The structural unity of the sense organs is the morphological parallel to their identity of physiological function, which, as we have seen, is to be impressed by certain modes of motion; and they are fine or coarse, in proportion to the delicacy or the strength of the impulses by which they are to be affected.

In ultimate analysis, then, it appears that a sensation is the equivalent in terms of consciousness for a mode of motion of the matter of the sensorium. But, if inquiry is pushed a stage farther, and the question is asked, What then do we know about matter and motion? there is but one reply possible. All that we know about motion is that it is a name for certain changes in the relations of our visual, tactile, and muscular sensations; and all that we know about matter is that it is the hypothetical substance of physical phenomena—the assumption of the existence of which is as pure a piece of metaphysical speculation as is that of the existence of the substance of mind.

Our sensations, our pleasures, our pains, and the relations of these, make

up the sum total of the elements of positive, unquestionable knowledge. We call a large section of these sensations and their relations matter and motion; the rest we term mind and thinking; and experience shows that there is a certain constant order of succession between some of the former and some of the latter.

This is all that just metaphysical criticism leaves of the idols set up by the spurious metaphysics of vulgar common sense. It is consistent either with pure Materialism, or with pure Idealism, but it is neither. For the Idealist, not content with declaring the truth that our knowledge is limited to facts of consciousness, affirms the wholly unprovable proposition that nothing exists beyond these and the substance of mind. And, on the other hand, the Materialist, holding by the truth that, for anything that appears to the contrary, material phenomena are the causes of mental phenomena, asserts his unprovable dogma, that material phenomena and the substance of matter are the sole primary existences.

Strike out the propositions about which neither controversialist does or can know anything, and there is nothing left for them to quarrel about. Make a desert of the Unknowable, and the divine Astræa of philosophic peace will commence her blessed reign.

V.

ON CERTAIN ERRORS RESPECTING THE STRUCTURE OF THE HEART ATTRIBUTED TO ARISTOTLE.

In all the commentaries upon the "Historia Animalium" which I have met with, Aristotle's express and repeated statement, that the heart of man and the largest animals contains only three cavities, is noted as a remarkable error. Even Cuvier, who had a great advantage over most of the commentators in his familiarity with the subject of Aristotle's description, and whose habitual caution and moderation seem to desert him when the opportunity of panegyrizing the philosopher presents itself, is betrayed into something like a sneer on this topic. "He gives to that organ only three cavities—an error which at least shows that he had observed its structure." ("Histoire des Sciences Naturelles," i. p. 152.)

To which remark, what follows will, I think, justify the reply, that it "at least shows" that Cuvier had not given ordinary attention, to say nothing of the careful study which they deserve, to sundry passages in the first and the third books of the "Historia" which I proceed to lay before the reader.

For convenience of reference these passages are marked *A*, *B*, *C*, etc.*

Book i. 17.—(*A*) "The heart has three cavities, it lies above the lung on the division of the windpipe, and has a fatty and thick membrane where it is united with the great vein and the aorta. It lies upon the aorta, with its point down the chest, in all animals that have a chest. In all, alike in those that have a chest and in those that have none, the foremost part of it is the apex. This is often overlooked through the turning upside down of the dissection. The rounded end of the heart is uppermost, the pointed end of it is largely fleshy and thick, and in its cavities there are tendons. In other animals which have a chest the heart lies in the middle of the chest; in men, more to the left side, between the nipples, a little inclined to the left nipple in the upper part of the chest. The heart is not large, and its general form is not elongated but rounded, except that the apex is produced into a point.

(*B*) "It has, as already stated, three cavities, the largest of them is on the right, the smallest on the left, the middle-sized one in the middle; they have all, also the two small ones, passages (τετρημένας) toward the lung, very evidently as respects one of the cavities. In the region of the union [with the great vein and the aorta] the largest cavity is connected with the largest vein (near which is the mesentery); the middle cavity with the aorta.

* The text I have followed is that given by Aubert and Wimmer, "Aristoteles Thierkunde: kritisch berichtigter Text mit deutschen Uebersetzung;" but I have tried here and there to bring the English version rather closer to the original than the German translation, excellent as it is, seems to me to be.

(*C*) "Canals (πόροι) from the heart pass to the lung and divide in the same fashion as the windpipe does, closely accompanying those from the windpipe through the whole lung. The canals from the heart are uppermost.

(*D*) "No canal is common [to the branches of the windpipe and those of the vein] but through those parts of them which are in contact, the air passes in and they [the πόροι] carry it to the heart.

(*E*) "One of the canals leads to the right cavity, the other to the left.

(*F*) "Of all the viscera, the heart alone contains blood [in itself]. The lung contains blood, not in itself but in the veins, the heart in itself; for in each of the cavities there is blood; the thinnest is in the middle cavity.

Book iii. 3.—(*G*) "Two veins lie in the thorax alongside the spine, on its inner face; the larger more forward, the smaller behind; the larger more to the right, the smaller, which some call *aorta* (on account of the tendinous part of it seen in dead bodies), to the left. These take their origin from the heart; they pass entire, preserving the nature of veins, through the other viscera that they reach; while the heart is rather a part of them, and more especially of the anterior and larger one, which is continued into veins above and below, while between these is the heart.

(*H*) "All hearts contain cavities, but in those of very small animals, the largest [cavity] is hardly visible, those of middling size have another, and the biggest all three.

(*I*) "The point of the heart is directed forward, as was mentioned at first; the largest cavity to the right and upper side of it, the smallest to the left, and the middle-sized one between these; both of these are much smaller than the largest.

(*K*) "They are all connected by passages (συντέτρηνται) with the lung, but, on account of the smallness of the canals, this is obscure except in one.

(*L*) "The great vein proceeds from the largest cavity which lies upward and to the right; next through the hollow middle part it becomes vein again, this cavity being a part of the vein in which the blood stagnates.

(*M*) "The aorta [proceeds from] the middle [cavity], but not in the same way, for it is connected [with the middle cavity] by a much more narrow tube.

(*N*) "The [great] vein extends through the heart, toward the aorta from the heart.

(*O*) "The great vein is membranous like skin, the aorta narrower than it and very tendinous, and as it extends toward the head and the lower parts it becomes narrow and altogether tendinous.

(*P*) "In the first place, a part of the great vein extends upward from the heart toward the lung and the attachment of the aorta, the vein being large and undivided. It divides into two parts, the one to the lung, the other to the spine and the lowest vertebra of the neck.

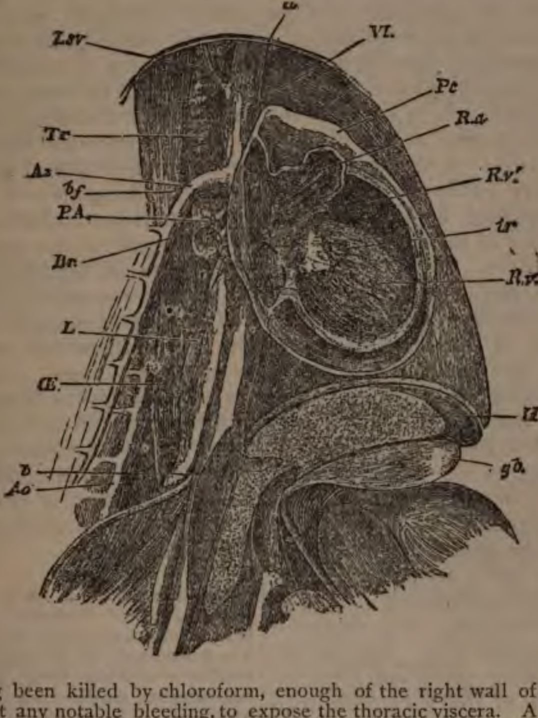
(*Q*) "The vein which extends to the lung first divides into two parts for the two halves of it and then extends alongside each tube, and each passage, the larger beside the larger and the smaller beside the smaller, so that no part [of the lung] can be found from which a passage, and a vein are absent. The terminations are invisible on account of their minuteness, but the whole lung appears full of blood. The canals from the vein lie above the tubes given off from the windpipe."

The key to the whole of the foregoing description of the heart lies in the passages (*G*) and (*L*). They prove that Aristotle, like Galen, five hundred years afterward, and like the great majority of the old Greek anatomists, did not reckon what we call the right auricle as a constituent of the heart at all, but as a hollow part, or dilatation, of the "great vein." Aristotle is careful to state that his observations were conducted on suffocated animals; and if any one will lay open the thorax of a dog or a rabbit, which has been killed with chloroform, in such a manner as to avoid wounding any important vessel, he will at once see why Aristotle adopted this view.

For, as the subjoined figure (p. 45) shows, the vena cava inferior (*b*), the right auricle (*R.a.*) and the vena cava superior and innominate vein (*V.I.*) distended with blood seem to form one continuous column, to which the heart is attached as a sort of appendage. This column is, as Aristotle says, vein above (*a*) and vein below (*b*), the upper and the lower divisions being connected by means of the intervening cavity or chamber (*R.a.*)—which is that which we call the right auricle.

But when, from the four cavities of the heart recognized by us moderns, one is excluded, there remain three—which is just what Aristotle says. The solution of the difficulty is, in fact, as absurdly simple as that presented by the egg of Columbus; and any error there may be, is not to be put down to Aristotle, but to that inability to comprehend that the same fact may be accurately described in different ways, which is the special characteristic of the commentatorial

mind. That the three cavities mentioned by Aristotle are just those which remain if the right auricle is omitted, is plain enough from what is said in (*B*), (*C*), (*E*), (*I*), and (*L*). For, in a suffocated animal, the "right cavity" which is directly connected with the great vein, and is obviously the right ventricle, being distended with blood, will look much larger than the middle cavity, which, since it gives rise to the aorta, can only be the left ventricle. And this, again, will appear larger than the thin and collapsed left auricle, which must be Aristotle's left cavity, inasmuch as this cavity is said to be connected by πόροι with the lung. The reason why Aristotle considered the left auricle to be a part of the heart, while he merged the right auricle in the great vein, is, obviously, the small relative size of the venous trunks and their sharper demarcation from the auricle. Galen, however, perhaps more consistently, regarded the left auricle also as a



A dog having been killed by chloroform, enough of the right wall of the thorax was removed, without any notable bleeding, to expose the thoracic viscera. A carefully measured outline sketch of the parts *in situ* was then made, and on dissection, twenty-four hours afterward, the necessary anatomical details were added. The woodcut is a faithfully reduced copy of the drawing thus constructed; and it represents the relations of the heart and great vessels as Aristotle saw them in a suffocated animal.

All but the inner lobe of the right lung has been removed; as well as the right half of the pericardium and the right walls of the right auricle and ventricle. It must be remembered that the thin transparent pericardial membrane appears nothing like so distinct in nature.

a.b., Aristotle's "great vein"; *V.I.*, right vena innominata and vena cava superior; *b*, the inferior vena cava; *R.a.*, the "hollow middle" part of the great vein or the right auricle; *R.v'*, the prolongation of the cavity of the right ventricle *R.v* towards the pulmonary artery; *tr*, one of the tricuspid valves; *Pc*, the pericardium; *I.sv*, superior intercostal vein; *Az*, vena azygos; *P.A.*, right pulmonary artery; *Br*, right bronchus; *L*, inner lobe of the right lung; *Œ*, œsophagus; *Ao*, descending aorta; *H*, liver, in section, with hepatic vein, vena portæ, and gall-bladder, *gb*, separated by the diaphragm, also seen in section, from the thoracic cavity.

mere part of the "arteria venosa." The canal which leads from the right cavity of the heart to the lung (or, as Aristotle puts it (*E*), from the lung to the heart) is, without doubt, the pulmonary artery. But it may be said that, in this case, Aristotle contradicts himself, inasmuch as in (*P*) and (*Q*) a vessel, which is obviously the pulmonary artery, is described as a branch of the great vein. However, this difficulty also disappears, if we reflect that, in Aristotle's way of looking at the matter, the line of demarkation between the great vein and the heart coincides with the right auriculo-ventricular aperture; and that, inasmuch as the conical prolongation of the right ventricle which leads to the pulmonary artery (*R.v'* in the Figure), lies close in front of the auricle, its base may very easily (as the figure shows) be regarded as a part of the general opening of the great vein into the right ventricle. In fact, it is clear that Aristotle, having failed to notice the valves of the heart, did not distinguish the part of the right ventricle from which the pulmonary artery arises (*R.v'*) from the proper trunk of the artery on the one hand, and from the right auricle (*R.a.*) on the other. Thus the root, as we may call it, of the pulmonary artery and the right auricle, taken together, are spoken of as the "part of the great vein which extends upward" (*P*); and, as the vena-azygos (*Az*) was one branch of this, so the "vein to the lung" was regarded as another branch of it. But the latter branch, being given off close to the connection of the great vein with the ventricle, was also counted as one of the two ways by which the "heart" (that is to say the right ventricle, the left ventricle, and the left auricle of our nomenclature) communicates with the lung.

The only other difficulty that I observe is connected with (*K*). If Aristotle intended by this to affirm that the middle cavity (the left ventricle), like the other two, is directly connected with the lung by a way, he would be in error. But he has excluded this interpretation of his words by (*E*), in which the number and relations of the canals, the existence of which he admits, are distinctly defined. I can only imagine then, that, so far as this passage applies to the left ventricle, it merely refers to the indirect communication of that cavity with the vessels of the lungs, through the left auricle.

On this evidence I submit that there is no escape from the conclusion that, instead of having committed a gross blunder, Aristotle has given a description of the heart which, so far as it goes, is remarkably accurate. He is in error only in regard to the differences which he imagines to exist between large and small hearts (*H*).

Cuvier (who has been followed by other commentators) ascribes another error to Aristotle:—"Aristotle supposed that the trachea, the windpipe, is prolonged to the heart, and seems to believe in consequence that the air penetrates thither" (l. c. p. 152).

Upon what foundation Cuvier rested the first of these two assertions, I am at a loss to divine. As a matter of fact, it will appear from the following excerpts that Aristotle gives an account of the structure of the lungs which is almost as good as that of the heart, and that it contains nothing about any prolongation of the windpipe to the heart.

"Within the neck lie what is called the œsophagus (so named on account of its length and its narrowness) and the windpipe. The position of the windpipe in all animals that have one, is in front of the œsophagus. All animals which possess a lung have a windpipe. The windpipe is of a cartilaginous nature and is exsanguine but is surrounded by many little veins. . . .

"It goes downward toward the middle of the lung, and then divides for each of the halves of the lung. In all animals that possess one, the lung is divided into two parts: but, in those which bring forth their young alive, the separation is not equally well marked, least of all in man.

"In oviparous animals, such as birds, and in quadrupeds which are oviparous, the one half of the lung is widely separated from the other; so that it appears as if they had two lungs. And from being single, the windpipe becomes (divided into) two, which extend to each half of the lung. It is fastened to the

great vein, and to what is called the aorta. When the windpipe is blown up, the air passes into the hollow parts of the lung. In these, are cartilaginous tubes (διαφύσεις) which unite at an angle; from the tubes passages (τρήματα) traverse the whole of the lung; they are continually given off, the smaller from the larger." (Book i. 16.)

That Aristotle should speak of the lung as a single organ divided into two halves, and should say that the division is least marked in man, is puzzling at first; but the statement becomes intelligible, if we reflect upon the close union of the bronchi, the pulmonary vessels and the mediastinal walls of the pleuræ, in mammals;* and it is quite true that the lungs are much more obviously distinct from one another in birds.

Aubert and Wimmer translate the last paragraph of the passage just cited as follows :—

"Diese haben aber knorpelige Scheidewände, welche unter spitzen Winkeln zusammentreten, und aus ihnen führen Oeffnungen durch die ganze Lunge, indem sie sich in immer kleineren verzweigen."

But I cannot think that by διαφύσεις and τρήματα, in this passage, Aristotle meant either "partitions" or openings in the ordinary sense of the latter word. For, in Book iii. Cap. 3, in describing the distribution of the "vein which goes to the lung" (the pulmonary artery), he says that it

"extends alongside each tube (σύριγγα) and each passage (τρῆμα), the larger beside the larger, and the smaller beside the smaller; so that no part (of the lung) can be found from which a passage (τρῆμα) and a vein are absent."

Moreover, in Book i. 17, he says—

"Canals (πόροι) from the heart pass to the lung and divide in the same fashion as the windpipe does, closely accompanying those from the windpipe through the whole lung."

And again in Book i. 17—

"It (the lung) is entirely spongy, and alongside of each tube (σύριγγα) run canals (πόροι) from the great vein."

On comparing the last three statements with the facts of the case, it is plain that by σύριγγες, or tubes, Aristotle means the bronchi and so many of their larger divisions as obviously contain cartilages; and that by διαφύσεις χονδρώδεις he denotes the same things; and, if this be so, then the τρήματα must be the smaller bronchial canals, in which the cartilages disappear.

This view of the structure of the lung is perfectly correct so far as it extends; and, bearing it in mind, we shall be in a position to understand what Aristotle thought about the passage of air from the lungs into the heart. In every part of the lung, he says, in effect, there is an air tube which is derived from the trachea, and other tubes which are derived from the πόροι which connect the lung with the heart (*suprà, C*). Their applied walls constitute the thin "synapses" (τὴν σύναψιν) through which the air passes out of the air tubes into the πόροι, or blood vessels, by transudation or diffusion; for there is no community between the cavities of the air tubes and cavities of the canals; that that is to say, no opening from one into the other (*suprà, D*).

On the words "κοινὸς πόρος" Aubert and Wimmer remark (*l. c.* p. 239), "Da A. die Ansicht hat die Lungenluft würde dem Herzen zugeführt, so postulirt er statt vieler kleiner Verbindungen einen grossen Verbindungsgang zwischen Lunge und Herz."

But does Aristotle make this assumption? The only evidence so far as I know in favor of the affirmative answer to this question is the following passage :—

"The heart and the windpipe are connected by fatty and cartilaginous and fibrous bands; where they are connected it is hollow. Blowing into the windpipe does not show clearly in some animals, but in the larger animals it is clear that the air goes into it." (i. cap. 16.)

Aubert and Wimmer give a some-

* In modern works on Veterinary Anatomy the lungs are sometimes described as two lobes of a single organ.

what different rendering of this passage :—

"Auch das Herz hängt mit der Luftröhre durch fettreiche, knorpelige und faserige Bänder zusammen; und da, wo sie zusammenhängen, ist eine Höhlung. Beim Aufblasen der Lunge wird es bei manchen Thieren nicht wahrnehmbar, bei den grösseren aber ist es offenbar, dass die Luft in das Herz gelangt."

The sense here turns upon the signification which is to be ascribed to *into it* (εἰς αὐτὴν). But if these words refer to the heart, then Aristotle has distinctly pointed out the road which the air, in his opinion, takes, namely, through the "synapses" (*D*); and there is no reason that I can discover to believe that he "postulated" any other and more direct communication.

With respect to the meaning of κοῖλόν ἐστιν (*it is hollow*), Aubert and Wimmer observe :—

"Dies scheint wohl die kurze Lungenvene zu sein. Schneider bezieht dies auf die Vorkammern, allein diese werden unten als Höhlen des Herzens beschrieben."

I am disposed to think, on the contrary, that the words refer simply to the cavity of the pericardium. For a part of this cavity (*sinus transversus pericardii*) lies between the aorta, on the one hand, and the pulmonary vessels with the bifurcation of the trachea, on the other hand, and is much more conspicuous in some animals than in man. It is strictly correct, therefore, in Aristotle's words, to say that where the heart and the windpipe are connected "it is hollow." If he had meant to speak of one of the pulmonary veins, or of any of the cavities of the heart, he would have used the terms [illegible] or [illegible] which he always employs for these parts.

According to Aristotle, then, the air taken into the lungs passes, from the final ramifications of the bronchial tubes into the corresponding branches of the pulmonary blood-vessels, not through openings, but by transudation, or, as we should nowadays say, diffusion, through the thin partitions formed by the applied coats of the two sets of canals. But the "pneuma" which thus reached the interior of the blood-vessels was not, in Aristotle's opinion, exactly the same thing as the air. It was "ἀὴρ πολὺς ῥέων καὶ ἀθρόος" ("De Mundo," iv. 9)—subtilized and condensed air; and it is hard to make out whether Aristotle considered it to possess the physical properties of an elastic fluid or those of a liquid. As he affirms that all the cavities of the heart contain blood (*F*), it is clear that he did not hold the erroneous view propounded in the next generation by Erasistratus. On the other hand, the fact that he supposes that the spermatic arteries do not contain blood but only an αἱματώδης ὑγρόν ("Hist. Animalium," iii. 1), shows that his notions respecting the contents of the arteries were vague. Nor does he seem to have known that the pulse is characteristic only of the arteries; and as he thought that the arteries end in solid fibrous bands, he naturally could not have entertained the faintest conception of the true motion of the blood. But, without attempting to read into Aristotle modern conceptions which never entered his mind, it is only just to observe that his view of what becomes of the air taken into the lungs is by no means worthy of contempt as a gross error. On the contrary, here, as in the case of his anatomy of the heart, what Aristotle asserts is true as far as it goes. Something does actually pass from the air contained in the lungs through the coats of the vessels into the blood, and thence to the heart: to wit, oxygen. And I think that it speaks very well for ancient Greek science that the investigator of so difficult a physiological problem as that of respiration, should have arrived at a conclusion, the statement of which, after the lapse of more than two thousand years, can be accepted as a thoroughly established scientific truth.

I trust that the case in favor of removing the statements about the heart from the list of the "errors of

Aristotle" is now clear; and that the evidence proves, on the contrary, that they justify us in forming a very favorable estimate of the oldest anatomical investigations among the Greeks of which any sufficient record remains.

But is Aristotle to be credited with the merit of having ascertained so much of the truth? This question will not appear superfluous to those who are acquainted with the extraordinary history of Aristotle's works, or who adopt the conclusion of Aubert and Wimmer, that, of the ten books of the "Historia Animalium" which have come down to us, three are largely or entirely spurious, and that the others contain many interpolations by later writers.

It so happens, however, that, apart from other reasons, there are satisfactory internal grounds for ascribing the account of the heart to a writer of the time at which Aristotle lived.

For, within thirty years of his death, the anatomists of the Alexandrian school had thoroughly investigated the structure and the functions of the valves of the heart. During this time the manuscripts of Aristotle were in the possession of Theophrastus; and no interpolator of later date would have shown that he was ignorant of the nature and significance of these important structures, by the brief and obscure allusion—"in its cavities there are tendons" (*A*). On the other hand, Polybus, whose account of the vascular system is quoted in the "Historia Animalium," was an elder contemporary of Aristotle. Hence, if any part of the work faithfully represents that which Aristotle taught, we may safely conclude that the description of the heart does so. Having granted this much, however, it is another question, whether Aristotle is to be regarded as the first discoverer of the facts which he has so well stated, or whether he, like other men, was the intellectual child of his time and simply carried on a step or two the work which had been commenced by others.

On the subject of Aristotle's significance as an original worker in biology extraordinarily divergent views have been put forward. If we are to adopt Cuvier's estimate, Aristotle was simply a miracle:—

"Before Aristotle, philosophy, being entirely speculative, lost itself in baseless abstractions: science did not exist. Science would seem to have sprung completely forward from the brain of Aristotle, as Minerva sprung fully armed from the brain of Jupiter. Indeed this one man, without predecessors, without borrowing aught from the ages that went before, as they had produced nothing of solid merit, discovered and demonstrated a greater number of truths, performed more scientific work in a lifetime of 62 years than 20 centuries have been able to perform since," etc. "Aristotle was the first to introduce the method of induction, comparison and observation, in order to reach general ideas, and the method of experiment in order to multiply the facts from which these general ideas may be deduced." ("Hist. des Sciences Nat." t. i. p. 130; t. ii. p. 515.)

The late Mr. G. H. Lewes, on the contrary, tells us "on a superficial examination, therefore, he [Aristotle] will seem to have given tolerable descriptions; especially if approached with that disposition to discover marvels which unconsciously determines us in our study of eminent writers. But a more unbiased and impartial criticism will disclose that he has given no single anatomical description of the least value. All that he knew may have been known, and probably was known, without dissection. . . . I do not assert that he never opened an animal; on the contrary it seems highly probable that he had opened many. . . . He never followed the course of a vessel or a nerve; never laid bare the origin and insertion of a muscle; never discriminated the component parts of organs; never made clear to himself the connection of organs into systems."—("Aristotle, a Chapter from the History of Science," pp. 156–7.)

In the face of the description of the heart and lungs, just quoted, I think we may venture to say that no one who has acquired even an elementary practical acquaintance with anatomy, and knows of his own knowledge that

which Aristotle describes, will agree with the opinion expressed by Mr. Lewes; and those who turn to the accounts of the structure of the rock lobster and the common lobster, or to that of the Cephalopods and other Mollusks, in the fourth book of the "Historia Animalium," will probably feel inclined to object to it still more strongly.

On the other hand, Cuvier's exaggerated panegyric will as little bear the test of cool discussion. In Greece, the century before Aristotle's birth was a period of great intellectual activity, in the field of physical science no less than elsewhere. The method of induction has never been used to better effect than by Hippocrates; and the labors of such men as Alkmeon, Demokritus, and Polybus, among Aristotle's predecessors; Diokles, and Praxagoras, among his contemporaries, laid a solid foundation for the scientific study of anatomy and development, independently of his labors. Aristotle himself informs us that the dissection of animals was commonly practiced; that the aorta had been distinguished from the great vein; and that the connection of both with the heart had been observed by his predecessors. What they thought about the structure of the heart itself or that of the lungs, he does not tell us, and we have no means of knowing. So far from arrogantly suggesting that he owed nothing to his predecessors, Aristotle is careful to refer to their observations, and to explain why, in his judgment, they fell into the errors which he corrects.

Aristotle's knowledge, in fact, appears to have stood in the same relation to that of such men as Polybus and Diogenes of Apollonia, as that of Herophilus and Erasistratus did to his own, so far as the heart is concerned. He carried science a step beyond the point at which he found it; a meritorious, but not a miraculous, achievement. What he did, required the possession of very good powers of observation; if they had been powers of the highest class, he could hardly have left such conspicuous objects as the valves of the heart to be discovered by his successors.

And this leads me to make a final remark upon a singular feature of the "Historia Animalium." As a whole, it is a most notable production, full of accurate information, and of extremely acute generalizations of the observations accumulated by naturalists up to that time. And yet, every here and there, one stumbles upon assertions respecting matters which lie within the scope of the commonest inspection, which are not so much to be called errors, as stupidities. What is to be made of the statement that the sutures of women's skulls are different from those of men; that men and sundry male animals have more teeth than their respective females; that the back of the skull is empty; and so on? It is simply incredible to me, that the Aristotle who wrote the account of the heart, also committed himself to absurdities which can be excused by no theoretical prepossession and which are contradicted by the plainest observation.

What, after all, were the original manuscripts of the "Historia Animalium"? If they were notes of Aristotle's lectures taken by some of his students, any lecturer who has chanced to look through such notes, would find the interspersion of a foundation of general and sometimes minute accuracy, with patches of transcendent blundering, perfectly intelligible. Some competent Greek scholar may perhaps think it worth while to tell us what may be said for or against the hypothesis thus hinted. One obvious difficulty in the way of adopting it is the fact that, in other works, Aristotle refers to the "Historia Animalium" as if it had already been made public by himself.

LECTURES ON EVOLUTION.

BY

THOMAS H. HUXLEY.

CONTENTS.

LECTURES ON EVOLUTION:

WITH AN APPENDIX ON

THE STUDY OF BIOLOGY.

BY THOMAS H. HUXLEY.

LECTURE I.*

The Three Hypotheses Respecting the History of Nature.

We live in and form part of a system of things of immense diversity and perplexity, which we call Nature; and it is a matter of the deepest interest to all of us that we should form just conceptions of the constitution of that system and of its past history. With relation to this universe, man is in extent, little more than a mathematical point; in duration but a fleeting shadow; he is a mere reed, shaken in the winds of force. But, as Pascal long ago remarked, although a mere reed, he is a thinking reed; and in virtue of that wonderful capacity of thought, he has the power of framing for himself a symbolic conception of the universe, which, although doubtless highly imperfect and inadequate as a picture of the great whole, is yet sufficient to serve him as a chart for the guidance of his practical affairs. It has taken long ages of toilsome and often fruitless labor to enable man to look steadily at the shifting scenes of the phantasmagoria of Nature, to notice what is fixed among her fluctuations, and what is regular among her apparent irregularities; and it is only comparatively lately, within the last few centuries, that the conception of a universal order and of a definite course of things, which we term the course of Nature, has emerged.

But, once originated, the conception of the constancy of the order of Nature has become the dominant idea of modern thought. To persons familiar with the facts upon which that conception is based, and competent to estimate their significance, it has ceased to be conceivable that

* These Lectures were delivered in New York Sept. 18, 20, 22, 1876.

chance should have any place in the universe, or that events should depend upon any but the natural sequence of cause and effect. We have come to look upon the present as the child of the past and as the parent of the future; and, as we have excluded chance from a place in the universe, so we ignore, even as a possibility, the notion of any interference with the order of nature. Whatever may be men's speculative doctrines, it is quite certain that every intelligent person guides his life and risks his fortune upon the belief that the order of nature is constant, and that the chain of natural causation is never broken.

In fact, no belief which we entertain has so complete a logical basis as that to which I have just referred. It tacitly underlies every process of reasoning; it is the foundation of every act of the will. It is based upon the broadest induction, and it is verified by the most constant, regular, and universal of deductive processes. But we must recollect that any human belief, however broad its basis, however defensible it may seem, is, after all, only a probable belief, and that our widest and safest generalizations are simply statements of the highest degree of probability. Though we are quite clear about the constancy of the order of Nature, at the present time, and in the present state of things, it by no means necessarily follows that we are justified in expanding this generalization into the infinite past, and in denying, absolutely, that there may have been a time when Nature did not follow a fixed order, when the relations of cause and effect were not definite, and when extra-natural agencies interfered with the general course of Nature. Cautious men will allow that a universe so different from that which we know may have existed; just as a very candid thinker may admit that a world in which two and two do not make four, and in which two straight lines do enclose a space, may exist. The same caution which forces the admission of such possibilities demands a great deal of evidence before it recognizes them to be anything more substantial. And when it is asserted that, so many thousand years ago, events occurred in a manner utterly foreign to and inconsistent with the existing laws of Nature, men, who without being particularly cautious, are simply honest thinkers, unwilling to deceive themselves or delude others, ask for trustworthy evidence of the fact.

Did things so happen or did they not? This is a historical question, and one the answer to which must be sought in the same way as the solution of any other historical problem.

So far as I know, there are only three hypotheses which ever have been entertained, or which well can be entertained, respecting the past history of Nature. I will, in the first place, state the hypotheses, and then I will consider what evidence bearing upon them is in our possession, and by what light of criticism that evidence is to be interpreted.

Upon the first hypothesis, the assumption is, that phenomena of Nature similar to those exhibited by the present world have always existed; in other words, that the universe has existed from all eternity in what may be broadly termed its present condition.

The second hypothesis is, that the present state of things has had only a limited duration; and that, at some period in the past, a condition of the world, essentially similar to that which we now know, came into existence, without any precedent condition from which it would have naturally proceeded. The assumption that successive states of Nature have arisen, each without any relation of natural causation to an antecedent state, is a mere modification of this second hypothesis.

The third hypothesis also assumes that the present state of things has had but a limited duration; but it supposes that this state has been evolved by a natural process from an antece-

dent state, and that from another, and so on; and, on this hypothesis, the attempt to assign any limit to the series of past changes is, usually, given up.

It is so needful to form clear and distinct notions of what is really meant by each of these hypotheses that I will ask you to imagine what, according to each, would have been visible to a spectator of the events which constitute the history of the earth. On the first hypothesis, however far back in time that spectator might be placed, he would see a world essentially, though perhaps not in all its details, similar to that which now exists. The animals which existed would be the ancestors of those which now live, and similar to them; the plants, in like manner, would be such as we know; and the mountains, plains, and waters would foreshadow the salient features of our present land and water. This view was held more or less distinctly, sometimes combined with the notion of recurrent cycles of change, in ancient times; and its influence has been felt down to the present day. It is worthy of remark that it is a hypothesis which is not inconsistent with the doctrine of Uniformitarianism, with which geologists are familiar. The doctrine was held by Hutton, and in his earlier days by Lyell. Hutton was struck by the demonstration of astronomers that the perturbations of the planetary bodies, however great they may be, yet sooner or later right themselves; and that the solar system possesses a self-adjusting power by which these aberrations are all brought back to a mean condition. Hutton imagined that the like might be true of terrestrial changes; although no one recognized more clearly than he the fact that the dry land is being constantly washed down by rain and rivers and deposited in the sea; and that thus, in a longer or shorter time, the inequalities of the earth's surface must be levelled, and its high lands brought down tc the ocean. But, taking into account the internal forces of the earth, which, upheaving the sea-bottom, give rise to newl and, he thought that these operations of degradation and elevation might compensate each other; and that thus, for any assignable time, the general features of our planet might remain what they are. And inasmuch as, under these circumstances, there need be no limit to the propagation of animals and plants, it is clear that the consistent working-out of the uniformitarian idea might lead to the conception of the eternity of the world. Not that I mean to say that either Hutton or Lyell held this conception—assuredly not; they would have been the first to repudiate it. Nevertheless, the logical development of their arguments tends directly towards this hypothesis.

The second hypothesis supposes that the present order of things, at some no very remote time, had a sudden origin, and that the world, such as it now is, had chaos for its phenomenal antecedent. That is the doctrine which you will find stated most fully and clearly in the immortal poem of John Milton—the English *Divina Commedia—Paradise Lost.* I believe it is largely to the influence of that remarkable work, combined with the daily teachings to which we have all listened in our childhood, that this hypothesis owes its general wide diffusion as one of the current beliefs of English-speaking people. If you turn to the seventh book of *Paradise Lost*, you will find there stated the hypothesis to which I refer, which is briefly this: That this visible universe of ours came into existence at no great distance of time from the present; and that the parts of which it is composed made their appearance, in a certain definite order, in the space of six natural days, in such a manner that, on the first of these days, light appeared; that, on the second, the firmament, or sky, separated the waters above from the waters beneath the firmament; that, on the third day, the waters drew away from the dry land, and upon it

a varied vegetable life, similar to that which now exists, made its appearance; that the fourth day was signalized by the apparition of the sun, the stars, the moon, and the planets; that, on the fifth day, aquatic animals originated within the waters; that, on the sixth day, the earth gave rise to our four-footed terrestrial creatures, and to all varieties of terrestrial animals except birds, which had appeared on the preceding day; and, finally, that man appeared upon the earth, and the emergence of the universe from chaos was finished. Milton tells us, without the least ambiguity, what a spectator of these marvelous occurrences would have witnessed. I doubt not that his poem is familiar to all of you, but I should like to recall one passage to your minds, in order that I may be justified in what I have said regarding the perfectly concrete, definite picture of the origin of the animal world which Milton draws. He says:—

"The sixth, and of creation last, arose
With evening harps and matin, when God said,
'Let the earth bring forth soul living in her kind,
Cattle and creeping things, and beast of the earth,
Each in their kind!' The earth obeyed, and, straight
Opening her fertile womb, teemed at a birth
Innumerous living creatures, perfect forms,
Limbed and full-grown. Out of the ground uprose,
As from his lair, the wild beast, where he wons
In forest wild, in thicket, brake or den;
Among the trees in pairs they rose, they walked;
The cattle in the fields and meadows green;
Those rare and solitary: these in flocks
Pasturing at once, and in broad herds upsprung.
The grassy clods now calved; now half appears
The tawny lion, pawing to get free
His hinder parts—then springs, as broke from bonds,
And rampant shakes his brinded mane; the ounce,
The libbard, and the tiger, as the mole
Rising, the crumbled earth above them threw
In hillocks, the swift stag from underground
Bore up his branching head; scarce from his mould
Behemoth, biggest born of earth, upheaved
His vastness; fleeced the flocks and bleating rose
As plants; ambiguous between sea and land,
The river-horse and scaly crocodile,
At once came forth whatever creeps the ground,
Insect or worm."

There is no doubt as to the meaning of this statement, nor as to what a man of Milton's genius expected would have been actually visible to an eye-witness of this mode of origination of living things.

The third hypothesis, or the hypothesis of evolution, supposes that, at any comparatively late period of past time, our imaginary spectator would meet with a state of things very similar to that which now obtains; but that the likeness of the past to the present would gradually become less and less, in proportion to the remoteness of his period of observation from the present day; that the existing distribution of mountains and plains, of rivers and seas, would show itself to be the product of a slow process of natural change operating upon more and more widely different antecedent conditions of the mineral framework of the earth; until, at length, in place of that framework, he would behold only a vast nebulous mass, representing the constituents of the sun and of the planetary bodies. Preceding the forms of life which now exist, our observer would see animals and plants not identical with them, but like them; increasing their differences with their antiquity, and, at the same time, becoming simpler and simpler: until, finally, the world of life would present nothing but that undifferentiated protoplasmic matter which, so

far as our present knowledge goes, is the common foundation of all vital activity.

The hypothesis of evolution supposes that in all this vast progression there would be no breach of continuity, no point at which we could say "This is natural process," and "This is not a natural process;" but that the whole might be compared to that wonderful process of development which may be seen going on every day under our eyes, in virtue of which there arises, out of the semi-fluid, comparatively homogeneous substance which we call an egg; the complicated organization of one of the higher animals. That, in a few words, is what is meant by the hypothesis of evolution.

I have already suggested that in dealing with these three hypotheses, in endeavoring to form a judgment as to which of them is the more worthy of belief, or whether none is worthy of belief—in which case our condition of mind should be that suspension of judgment which is so difficult to all but trained intellects—we should be indifferent to all *a priori* considerations. The question is a question of historical fact. The universe has come into existence somehow or other, and the problem is, whether it came into existence in one fashion, or whether it came into existence in another; and as an essential preliminary to further discussion, permit me to say two or three words as to the nature and the kinds of historical evidence.

The evidence as to the occurrence of any event in past time may be ranged under two heads which, for convenience's sake, I will speak of as testimonial evidence and as circumstantial evidence. By testimonial evidence I mean human testimony, and by circumstantial evidence I mean evidence which is not human testimony. Let me illustrate by a familiar example what I understand by these two kinds of evidence, and what is to be said respecting their value.

Suppose that a man tells you that he saw a person strike another and kill him; that is testimonial evidence of the fact of murder. But it is possible to have circumstantial evidence of the fact of murder; that is to say, you may find a man dying with a wound upon his head having exactly the form and character of the wound which is made by an axe, and, with due care in taking surrounding circumstances into account, you may conclude with the utmost certainty that the man has been murdered; that his death is the consequence of a blow inflicted by another man with that inplement. We are very much in the habit of considering circumstantial evidence as of less value than testimonial evidence, and it may be that, where the circumstances are not perfectly clear and intelligible, it is a dangerous and unsafe kind of evidence; but it must not be forgotten that, in many cases, circumstantial is quite as conclusive as testimonial evidence, and that, not unfrequently, it is a great deal weightier than testimonial evidence. For example, take the case to which I referred just now. The circumstantial evidence may be better and more convincing than the testimonial evidence; for it may be impossible, under the conditions that I have defined, to suppose that the man met his death from any other cause but the violent blow of an axe, wielded by another man. The circumstantial evidence in favor of a murder having been committed, in that case, is as complete and as convincing as evidence can be. It is evidence which is open to no doubt and to no falsification. But the testimony of a witness is open to multitudinous doubts. He may have been mistaken. He may have been actuated by malice. It has constantly happened that even an accurate man has declared that a thing has happened in this, that, or the other way, when a careful analysis of the circumstantial evidence has shown that it did not happen in that way, but in some other way.

We may now consider the evidence in favor of or against the three hypotheses. Let me first direct your attention to what is to be said about the hypothesis of the eternity of the state of things in which we now live. What will first strike you is, that it is a hypothesis which, whether true or false, is not capable of verification by any evidence. For, in order to obtain either circumstantial or testimonial evidence sufficient to prove the eternity of duration of the present state of nature, you must have an eternity of witnesses or an infinity of circumstances, and neither of these is attainable. It is utterly impossible that such evidence should be carried beyond a certain point of time; and all that could be said, at most, would be, that so far as the evidence could be traced, there was nothing to contradict the hypothesis. But when you look, not to the testimonial evidence—which, considering the relative insignificance of the antiquity of human records, might not be good for much in this case—but to the circumstantial evidence, then you find that this hypothesis is absolutely incompatible with such evidence as we have; which is of so plain and simple a character that it is impossible in any way to escape from the conclusions which it forces upon us.

You are, doubtless, all aware that the outer substance of the earth, which alone is accessible to direct observation, is not of a homogeneous character, but that it is made up of a number of layers or strata, the titles of the principal groups of which are placed upon the accompanying diagram. Each of these groups represents a number of beds of sand, of stone, of clay, of slate, and of various other materials.

On careful examination, it is found that the materials of which each of these layers of more or less hard rock are composed are, for the most part, of the same nature as those which are at present being formed under known conditions on the surface of the earth. For example, the chalk, which constitutes a great part of the Cretaceous formation in some parts of the world, is practically identical in its physical and chemical characters with a substance which is now being formed at the bottom of the Atlantic Ocean, and covers an enormous area; other beds of rock are comparable with the sands which are being formed upon sea-shores, packed together, and so on. Thus, omitting rocks of igneous origin, it is demonstrable that all these beds of stone, of which a total of not less than seventy thousand feet is known, have been formed by natural agencies, either out of the waste and washing of the dry land, or else by the accumulation of the exuviæ of plants and animals. Many of these strata are full of such exuviæ—the so-called "fossils." Remains of thousand of species of animals and plants, as perfectly recognizable as those of existing forms of life which you meet with in museums, or as the shells which you pick up upon the sea-beech, have been imbedded in the ancient sands, or muds, or limestones, just as they are being imbedded now, in sandy, or clayey, or calcareous subaqueous deposits. They furnish us with a record, the general nature of which cannot be misinterpreted, of the kinds of things that have lived upon the surface of the earth during the time that is registered by this great thickness of stratified rocks. But even a superficial study of these fossils shows us that the animals and plants which live at the present time have had only a temporary duration; for the remains of such modern forms of life are met with, for the most part, only in the uppermost or latest tertiaries, and their number rapidly diminishes in the lower deposits of that epoch. In the older tertiaries, the places of existing animals and plants are taken by other forms, as numerous and diversified as those which live now in the same localities, but more or less different from them; in the mesozoic

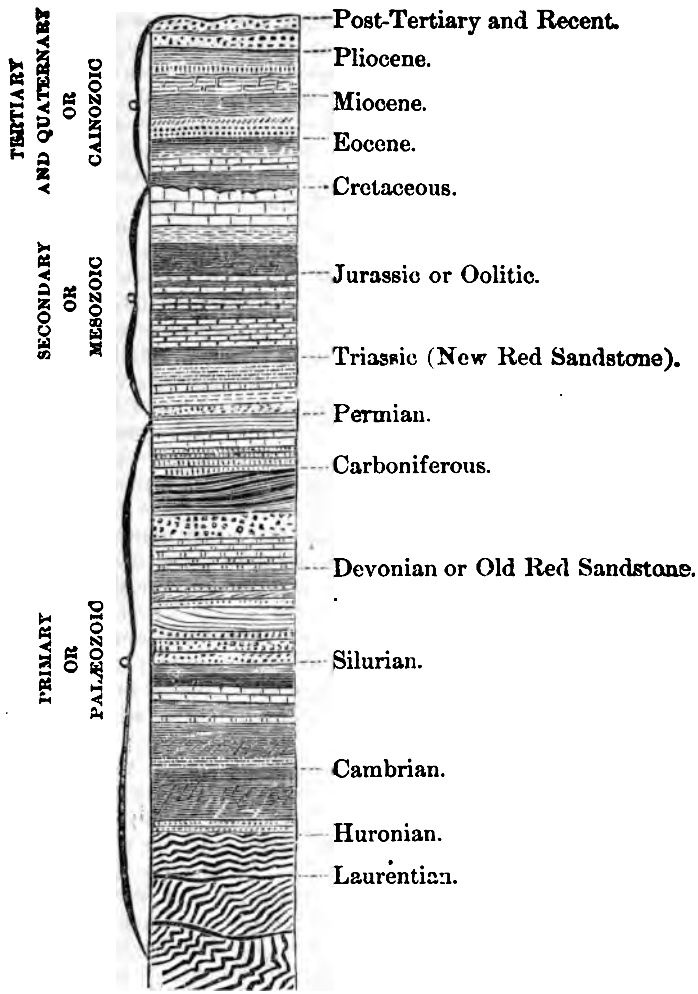


FIG. 1.—IDEAL SECTION OF THE CRUST OF THE EARTH.

rocks, these are replaced by others yet more divergent from modern types; and in the palæozoic formations the contrast is still more marked. Thus the circumstantial evidence absolutely negatives the conception of the eternity of the present condition of things. We can say with certainty that the present condition of things has existed for a comparatively short period; and that, so far as animal and vegetable nature are concerned, it has been preceded by a different condition. We can pursue this evidence until we reach the lowest of the stratified rocks, in which we lose the indications of life altogether. The hypothesis of the eternity of the present state of nature may therefore be put out of court.

We now come to what I will term Milton's hypothesis—the hypothesis that the present condition of things has endured for a comparatively short time; and, at the commencement of that time, came into existence within the course of six days.

I doubt not that it may have excited some surprise in your minds that I should have spoken of this as Milton's hypothesis, rather than that I should have chosen the terms which are more customary, such as "the doctrine of creation," or "the Biblical doctrine," or "the doctrine of Moses," all of which denominations, as applied to the hypothesis to which I have just referred, are certainly much more familar to you than the title of the Miltonic hypothesis. But I have had what I cannot but think are very weighty reasons for taking the course which I have pursued. In the first place, I have discarded the title of the "doctrine of creation," because my present business is not with the question why the objects which constitute Nature came into existence, but when they came into existence, and in what order. This is as strictly a historical question as the question when the Angles and the Jutes invaded England, and whether they preceded or followed the Romans. But the question about creation is a philosophical problem, and one which cannot be solved, or even approached, by the historical method. What we want to learn is, whether the facts, so far as they are known, afford evidence that things arose in the way described by Milton, or whether they do not; and, when that question is settled, it will be time enough to inquire into the causes of their origination.

In the second place, I have not spoken of this doctrine as the Biblical doctrine. It is quite true that persons as diverse in their general views as Milton the Protestant and the celebrated Jesuit Father Saurez, each put upon the first chapter of Genesis the interpretation embodied in Milton's poem. It is quite true that this interpretation is that which has been instilled into every one of us in our childhood; but I do not for one moment venture to say that it can properly be called the Biblical doctrine. It is not my business, and does not lie within my competency, to say what the Hebrew text does, and what it does not signify; moreover, were I to affirm that this is the Biblical doctrine, I should be met by the authority of many eminent scholars, to say nothing of men of science, who, at various times, have absolutely denied that any such doctrine is to be found in Genesis. If we are to listen to many expositors of no mean authority, we must believe that what seems so clearly defined in Genesis—as if very great pains had been taken that there should be no possibility of mistake—is not the meaning of the text at all. The account is divided into periods that we may make just as long or as short as convenience requires. We are also to understand that it is consistent with the original text to believe that the most complex plants and animals may have been evolved by natural processes, lasting for millions of years, out of structureless rudiments. A person who is not a Hebrew scholar can only stand aside and admire the marvelous flexibility of a language which admits of such diverse interpretations. But assuredly, in the face of such contradictions of authority upon matters respecting which he is incompetent to form any judgment, he will abstain, as I do, from giving any opinion.

In the third place, I have carefully abstained from speaking of this as the Mosaic doctrine, because we are now assured upon the authority of the highest critics, and even of dignitaries of the Church, that there is no evidence that Moses wrote the Book of Genesis, or knew anything about it. You will understand that I give no judgment—it would be an impertinence upon my part to volunteer even a suggestion—upon such a subject. But, that being the state of opinion among the scholars and the clergy, it is well for the unlearned in Hebrew lore, and for the laity, to avoid entangling themselves in such a vexed question. Happily, Milton leaves us no excuse for doubting what he means, and I shall therefore be safe in speaking of the opinion in

question as the Miltonic hypothesis.

Now we have to test that hypothesis. For my part I have no prejudice one way or the other. If there is evidence in favor of this view, I am burdened by no theoretical difficulties in the way of accepting it; but there must be evidence. Scientific men get an awkward habit—no, I won't call it that, for it is a valuable habit—of believing nothing unless there is evidence for it; and they have a way of looking upon belief which is not based upon evidence, not only as illogical, but as immoral. We will, if you please, test this view by the circumstantial evidence alone; for, from what I have said, you will understand that I do not propose to discuss the question of what testimonial evidence is to be adduced in favor of it. If those whose business it is to judge are not at one as to the authenticity of the only evidence of that kind which is offered, nor as to the facts to which it bears witness, the discussion of such evidence in superfluous.

But I may be permitted to regret this necessity of rejecting the testimonial evidence the less, because the examination of the circumstantial evidence leads to the conclusion, not only that it is incompetent to justify the hypothesis, but that, so far as it goes, it is contrary to the hypothesis.

The considerations upon which I base this conclusion are of the simplest possible character. The Miltonic hypothesis contains assertions of a very definite character relating to the succession of living forms. It is stated that plants, for example, made their appearance upon the third day, and not before. And you will understand that what the poet means by plants are such plants as now live, the ancestors, in the ordinary way of propagation of like by like, of the trees and shrubs which flourish in the present world. It must needs be so; for, if they were different, either the existing plants have been the result of a separate origination since that described by Milton, of which we have no record, nor any ground for supposition that such an occurrence has taken place; or else they have arisen by a process of evolution from the original stocks.

In the second place, it is clear that there was no animal life before the fifth day, and that, on the fifth day, aquatic animal and birds appeared. And it is further clear that terrestrial living things, other than birds, made their appearance upon the sixth day, and not before. Hence, it follows that, if, in the large mass of circumstantial evidence as to what really has happened in the past history of the globe we find indications of the existence of terrestrial animals, other than birds, at a certain period, it is perfectly certain that all that has taken place since that time must be referred to the sixth day.

In the great Carboniferous formation, whence America derives so vast a proportion of her actual and potential wealth, in the beds of coal which have been formed from the vegetation of that period, we find abundant evidence of the existence of terrestrial animals. They have been described, not only by European but by your own naturalists. There are to be found numerous insects allied to our cockroaches. There are to be found spiders and scorpions of large size, the latter so similar to existing scorpions that it requires the practiced eye of the naturalist to distinguish them. Inasmuch as these animals can be proved to have been alive in the Carboniferous epoch, it is perfectly clear that, if the Miltonic account is to be accepted, the huge mass of rocks extending from the middle of the Palæozoic formations to the uppermost members of the series, must belong to the day which is termed by Milton as the sixth. But, further, it is expressly stated that acquatic animals took their origin upon the fifth day, and not before; hence, all formations in which remains of acquatic animals can be proved to exist, and which therefore testify that such animals lived at the

time when these formations were in course of deposition, must have been deposited during or since the period which Milton speaks of as the fifth day. But there is absolutely no fossiliferous formation in which the remains of aquatic animals are absent. The oldest fossils in the Silurian rocks are exuviæ of marine animals; and if the view which is entertained by Principal Dawson and Dr. Carpenter respecting the nature of the *Eozoon* be well founded, aquatic animals existed at a period as far antecedent to the deposition of the coal as the coal is from us; inasmuch as the *Eozoon* is met with in those Laurentian strata which lie at the bottom of the series of stratified rocks. Hence it follows, plainly enough, that the whole series of stratified rocks, if they are to be brought into harmony with Milton, must be referred to the fifth and sixth days, and that we cannot hope to find the slightest trace of the products of the earlier days in the geological record. When we consider these simple facts, we see how absolutely futile are the attempts that have been made to draw a parallel between the story told by so much of the crust of the earth as is known to us and the story which Milton tells. The whole series of fossiliferous stratified rocks must be referred to the last two days: and neither the Carboniferous, nor any other, formation can afford evidence of the work of the third day.

Not only is there this objection to any attempt to establish a harmony between the Miltonic account and the facts recorded in the fossiliferous rocks, but there is a further difficulty. According to the Miltonic account, the order in which animals should have made their appearance in the stratified rocks would be this: Fishes, including the great whales, and birds; after them, all varieties of terrestrial animals except birds. Nothing could be further from the facts as we find them; we know of not the slightest evidence of the existence of birds before the Jurassic, or perhaps the Triassic, formation; while terrestrial animals, as we have just seen, occur in the Carboniferous rocks.

If there were any harmony between the Miltonic account and the circumstantial evidence, we ought to have abundant evidence of the existence of birds in the Carboniferous, the Devonian, and the Silurian rocks. I need hardly say that this is not the case, and that not a trace of birds makes its appearance until the far later period which I have mentioned.

And again, if it be true that all varieties of fishes and the great whales, and the like, made their appearance on the fifth day, we ought to find the remains of these animals in the older rocks—in those which were deposited before the Carboniferous epoch. Fishes we do find, in considerable number and variety; but the great whales are absent, and the fishes are not such as now live. Not one solitary species of fish now in existence is to be found in the Devonian or Silurian formations. Hence we are introduced afresh to the dilemma which I have already placed before you: either the animals which came into existence on the fifth day were not such as those which are found at present, are not the direct and immediate ancestors of those which now exist; in which case either fresh creations of which nothing is said; or a process of evolution must have occurred; or else the whole story must be given up, as not only devoid of any circumstantial evidence, but contrary to such evidence as exists.

I placed before you in a few words, some little time ago, a statement of the sum and substance of Milton's hypothesis. Let me now try to state as briefly, the effect of the circumstantial evidence bearing upon the past history of the earth which is furnished, without the possibility of mistake, with no chance of error as to its chief features, by the stratified rocks. What we find is, that the great series of formations represents a period of time of which our human chronologies hardly afford us a unit of measure. I will not pretend to say

how we ought to estimate this time, in millions or in billions of years. For my purpose, the determination of its absolute duration is wholly unessential. But that the time was enormous there can be no question.

It results from the simplest methods of interpretation, that leaving out of view certain patches of metamorphosed rocks, and certain volcanic products, all that is now dry land has once been at the bottom of the waters. It is perfectly certain that, at a comparatively recent period of the world's history—the Cretaceous epoch—none of the great physical features which at present mark the surface of the globe existed. It is certain that the Rocky Mountains were not. It is certain that the Himalaya Mountains were not. It is certain that the Alps and the Pyrenees had no existence. The evidence is of the plainest possible character; and is simply this: We find raised up on the flanks of these mountains, elevated by the forces of upheaval which have given rise to them, masses of Cretaceous rock which formed the bottom of the sea before those mountains existed. It is therefore clear that the elevatory forces which gave rise to the mountains operated subsequently to the Cretaceous epoch; and that the mountains themselves are largely made up of the materials deposited in the sea which once occupied their place. As we go back in time, we meet with constant alternations of sea and land, of estuary and open ocean; and, in correspondence with these alterations, we observe the changes in the fauna and flora to which I have referred.

But the inspection of these changes give us no right to believe that there has been any discontinuity in natural processes. There is no trace of general cataclysms, of universal deluges, or sudden destructions of a whole fauna or flora. The appearances which were formerly interpreted in that way have all been shown to be delusive, as our knowledge has increased and as the blanks which formerly appeared to exist between the different formations have been filled up. That there is no absolute break between formation and formation, that there has been no sudden disappearance of all the forms of life and replacement of them by others, but that changes have gone on slowly and gradually, that one type has died out and another has taken its place, and that thus, by insensible degrees, one fauna has been replaced by another, are conclusions strengthened by constantly increasing evidence. So that within the whole of the immense period indicated by the fossiliferous stratified rocks, there is assuredly not the slightest proof of any break in the uniformity of Nature's operations, no indication that events have followed other than a clear and orderly sequence.

That, I say, is the natural and obvious teaching of the circumstantial evidence contained in the stratified rocks. I leave you to consider how far, by an ingenuity of interpretation, by any stretching of the meaning of language, it can be brought into harmony with the Miltonic hypothesis.

There remains the third hypothesis, that of which I have spoken as the hypothesis of evolution; and I purpose that in lectures to come, we should discuss it as carefully as we have considered the other two hypotheses. I need not say that it is quite hopeless to look for testimonial evidence of evolution. The very nature of the case precludes the possibility of such evidence, for the human race can no more be expected to testify to its own origin than a child can be tendered as a witness of its own birth. Our sole inquiry is, what foundation circumstantial evidence lends to the hypothesis, or whether it lends none, or whether it controverts the hypothesis. I shall deal with the matter entirely as a question of history. I shall not indulge in the discussion of any speculative probabilities. I shall not attempt to show that Nature is unintelligible unless we adopt some such hypothesis. For

anything I know about the matter, it may be the way of Nature to be unintelligible; she is often puzzling, and I have no reason to suppose that she is bound to fit herself to our notions.

I shall place before you three kinds of evidence entirely based upon what is known of the forms of animal life which are contained in the series of stratified rocks. I shall endeavor to show you that there is one kind of evidence which is neutral, which neither helps evolution nor is inconsistent with it. I shall then bring forward a second kind of evidence which indicates a strong probability in favor of evolution, but does not prove it; and, lastly, I shall adduce a third kind of evidence which, being as complete as any evidence we can hope to obtain upon such a subject, and being wholly and strikingly in favor of evolution, may fairly be called demonstrative evidence of its occurrence.

LECTURE II.

The Hypothesis of Evolution.—The Neutral and the Favorable Evidence.

In the preceding lecture I pointed out that there are three hypotheses which may be entertained, and which have been entertained, respecting the past history of life upon the globe. According to the first of these hypotheses, living beings, such as now exist, have existed from all eternity upon this earth. We tested that hypothesis by the circumstantial evidence, as I called it, which is furnished by the fossil remains contained in the earth's crust, and we found that it was obviously untenable. I then proceeded to consider the second hypothesis, which I termed the Miltonic hypothesis, not because it is of any particular consequence to me whether John Milton seriously entertained it or not, but because it is stated in a clear and unmistakable manner in his great poem. I pointed out to you that the evidence at our command as completely and fully negatives that hypothesis as it did the preceding one. And I confess that I had too much respect for your intelligence to think it necessary to add that the negation was equally clear and equally valid, whatever the source from which that hypothesis might be derived, or whatever the authority by which it might be supported. I further stated that, according to the third hypothesis, or that of evolution, the existing state of things is the last term of a long series of states, which, when traced back, would be found to show no interruption and no breach in the continuity of natural causation. I propose, in the present, and the following lecture, to test this hypothesis rigorously by the evidence at command, and to inquire how far that evidence can be said to be indifferent to it, how far it can be said to be favorable to it, and, finally, how far it can be said to be demonstrative.

From almost the origin of the discussions about the existing condition of the animal and vegetable worlds and the causes which have determined that condition, an argument has been put forward as an objection to evolution, which we shall have to consider very seriously. It is an argument which was first clearly stated by Cuvier in his criticism of the doctrines propounded by his great contemporary, Lamarck. The French expedition to Egypt had called the attention of learned men to the wonderful store of antiquities in that country, and there had been brought back to France numerous mummified corpses of the animals which the ancient Egyptians revered and preserved, and which, at a reasonable computation, must have lived not less than three or four thousand years before the time at which they were thus brought to light. Cuvier endeavored to test the hypothesis that animals have undergone gradual and progressive modifications of structure, by comparing the skeletons and such other parts of the mummies as were

in a fitting state of preservation, with the corresponding parts of the representatives of the same species now living in Egypt. He arrived at the conviction that no appreciable change had taken place in these animals in the course of this considerable lapse of time, and the justice of his conclusion is not disputed.

It is obvious that, if it can be proved that animals have endured, without undergoing any demonstrable change of structure, for so long a period as four thousand years, no form of the hypothesis of evolution which assumes that animals undergo a constant and necessary progressive change can be tenable; unless, indeed, it be further assumed that four thousand years is too short a time for the production of a change sufficiently great to be detected.

But it is no less plain that if the process of evolution of animals is not independent of surrounding conditions; if it may be indefinitely hastened or retarded by variations in these conditions; or if evolution is simply a process of accomodation to varying conditions; the argument against the hypothesis of evolution based on the unchanged character of the Egyptian fauna is worthless. For the monuments which are coeval with the mummies testify as strongly to the absence of change in the physical geography and the general conditions of the land of Egypt, for the time in question, as the mummies do to the unvarying characters of its living population.

The progress of research since Cuvier's time has supplied far more striking examples of the long duration of the specific forms of life than those which are furnished by the mummified Ibises and Crocodiles of Egypt. A remarkable case is to be found in your own country, in the neighborhood of the falls of Niagara. In the immediate vicinity of the whirlpool, and again upon Goat Island, in the superficial deposits which cover the surface of the rocky subsoil in those regions, there are found remains of animals in perfect preservation, and among them, shells belonging to exactly the same species as those which at present inhabit the still waters of Lake Erie. It is evident, from the structure of the country, that these animal remains were deposited in the beds in which they occur at a time when the lake extended over the region in which they are found. This involves the conclusion that they lived and died before the falls had cut their way back through the gorge of Niagara; and, indeed, it has been determined that, when these animals lived, the falls of Niagara must have been at least six miles further down the river than they are at present. Many computasions have been made of the rate at which the falls are thus cutting their way back. Those computations have varied greatly, but I believe I am speaking within the bounds of prudence, if I assume that the falls of Niagara have not retreated at a greater pace than about a foot a year. Six miles, speaking roughly, are 30,000 feet; 30,000 feet, at a foot a year, gives 30,000 years; and thus we are fairly justified in concluding that no less a period than this has passed since the shell-fish, whose remains are left in the beds to which I have referred, were living creatures.

But there is still stronger evidence of the long duration of certain types. I have already stated that, as we work our way through the great series of the Tertiary formations, we find many species of animals identical with those which live at the present day, diminishing in numbers, it is true, but still existing, in a certain proportion, in the oldest of the Tertiary rocks. Furthermore, when we examine the rocks of the Cretaceous epoch, we find the remains of some animals which the closest scrutiny cannot show to be, in any important respect, different from those which live at the present time. That is the case with one of the cretaceous lamp-shells (*Terebratula*), which has continued to exist unchanged, or with

insignificant variations, down to the present day. Such is the case with the *Globigerinæ*, the skeletons of which aggregated together, form a large proportion of our English chalk. Those *Globigerinæ* can be traced down to the *Globigerinæ* which lived at the surface of the present great oceans, and the remains of which, falling to the bottom of the sea, give rise to a chalky mud. Hence it must be admitted that certain existing species of animals show no distinct sign of modification, or transformation, in the course of a lapse of time as great as that which carries us back to the Cretaceous period; and which, whatever its absolute measure, is certainly vastly greater than thirty thousand years.

There are groups of species so closely allied together that it needs the eye of a naturalist to distinguish them one from another. If we disregard the small differences which separate these forms and consider all the species of such groups as modifications of one type, we shall find that, even among the higher animals, some types have had a marvellous duration. In the chalk, for example, there is found a fish belonging to the highest and most differentiated group of osseous fishes, which goes by the name of *Beryx*. The remains of that fish are among the most beautiful and well preserved of the fossils found in our English chalk. It can be studied anatomically, so far as the hard parts are concerned, almost as well as if it were a recent fish. But the genus *Beryx* is represented, at the present day, by very closely allied species, which are living in the Pacific and Atlantic Oceans. We may still go farther back. I have already referred to the fact that the Carboniferous formations, in Europe and in America, contain the remains of scorpions in an admirable state of preservation, and that those scorpions are hardly distinguishable from such as now live. I do not mean to say that they are not different, but close scrutiny is needed in order to distinguish them from modern scorpions.

More than this. At the very bottom of the Silurian series, in beds which are by some authorities referred to the Cambrian formation, where the signs of life begin to fail us—even there, among the few and scanty animal remains which are discoverable, we find species of molluscous animals which are so closely allied to existing forms that, at one time, they were grouped under the same generic name. I refer to the well-known *Lingula* of the *Lingula* flags, lately, in consequence of some slight differences, placed in the new genus *Lingulella*. Practically, it belongs to the same great generic group as the *Lingula*, which is to be found at the present day upon your own shores and those of many other parts of the world.

The same truth is exemplified if we turn to certain great periods of the earth's history—as, for example, the Mesozoic epoch. There are groups of reptiles, such as the *Ichthyosauria* and *Plesiosauria*, which appear shortly after the commencement of this epoch, and they occur in vast numbers. They disappear with the chalk and, throughout the whole of the great Mesozoic rocks, they present no such modifications as can safely be considered evidence of progressive modification.

Facts of this kind are undoubtedly fatal to any form of the doctrine of evolution which postulates the supposition that there is an intrinsic necessity, on the part of animal forms which have once come into existence, to undergo continual modification; and they are as distinctly opposed to any view which involves the belief that such modification as may occur, must take place, at the same rate, in all the different types of animal or vegetable life. The facts, as I have placed them before you, obviously indirectly contradict any form of the hypothesis of evolution which stands in need of these two postulates.

But, one great service that has

been rendered by Mr. Darwin to the doctrine of evolution in general is this: he has shown that there are two chief factors in the process of evolution: one of them is the tendency to vary, the existence of which in all living forms may be proved by observation; the other is the influence of surrounding conditions upon what I may call the parent form and the variations which are thus evolved from it. The cause of the production of variations is a matter not at all properly understood at present. Whether variation depends upon some intricate machinery—if I may use the phrase—of the living organism itself, or whether it arises through the influence of conditions upon that form, is not certain, and the question may, for the present, be left open. But the important point is that, granting the existence of the tendency to the production of variations; then, whether the variations which are produced shall survive and supplant the parent, or whether the parent form shall survive and supplant the variations, is a matter which depends entirely on those conditions which give rise to the struggle for existence. If the surrounding conditions are such that the parent form is more competent to deal with them and flourish in them than the derived forms, then, in the struggle for existence, the parent form will maintain itself and the derived forms will be exterminated. But if, on the contrary, the conditions are such as to be more favorable to a derived than to the parent form, the parent form will be extirpated and the derived form will take its place. In the first case, there will be no progression, no change of structure, through any imaginable series of ages; in the second place, there will be modification and change of form.

Thus the existence of these persistent types, as I have termed them, is no real obstacle in the way of the theory of evolution. Take the case of scorpions to which I have just referred. No doubt, since the Carboniferous epoch, conditions have always obtained, such as existed when the scorpions of that epoch flourished; conditions in which scorpions find themselves better off, more competent to deal with the difficulties in their way, than any variation from the scorpion type which they may have produced; and, for that reason, the scorpion type has persisted, and has not been supplanted by any other form. And there is no reason, in the nature of things, why, as long as this world exists, if there be conditions more favorable to scorpions than to any variation which may arise from them these forms of life shuld not persist.

Therefore, the stock objection to the hpothesis of evolution, based on the long duration of certain animal and vegetable types, is no objection at all. The facts of this character—and they are numerous—belong to that class of evidence which I have called indifferent. That is to say, they may afford no direct support to the doctrine of evolution, but they are capable of being interpreted in perfect consistency with it.

There is another order of facts belonging to the class of negative or indifferent evidence. The great group of Lizards, which abound in the present world, extends through the whole series of formations as far back as the Permian, or latest Palæozoic, epoch. These Permian lizards differ astonishingly little from the lizards which exist at the present day. Comparing the amount of the differences between them and modern lizards, with the prodigious lapse of time between the Permian epoch and the present age, it may be said that the amount of change is insignificant. But, when we carry our researches farther back in time, we find no trace of lizards, nor of any true reptile whatever, in the whole mass of formations beneath the Permian.

Now, it is perfectly clear that if our palæontological collections are to be taken, even approximately, as an adequate representation of all the forms of animals and plants that have

ever lived; and if the record furnished by the known series of beds of stratified rocks, covers the whole series of events which constitute the history of life on the globe, such a fact as this directly contravenes the hypothesis of evolution; because this hypothesis postulates that the existence of every form must have been preceded by that of some form little different from it. Here, however, we have to take into consideration that important truth so well insisted upon by Lyell and by Darwin—the imperfection of the geological record. It can be demonstrated that the geological record must be incomplete, that it can only preserve remains found in certain favorable localities and under particular conditions; that it must be destroyed by processes of denudation, and obliterated by processes of metamorphosis. Beds of rock of any thickness, crammed full of organic remains, may yet, either by the percolation of water through them, or by the influence of subterranean heat, lose all trace of these remains, and present the appearance of beds of rock formed under conditions in which living forms were absent. Such metamorphic rocks occur in formations of all ages; and, in various cases, there are very good grounds for the belief that they have contained organic remains, and that those remains have been absolutely obliterated.

I insist upon the defects of the geological record the more because those who have not attended to these matters are apt to say, "It is all very well, but when you get into a difficulty with your theory of evolution, you appeal to the incompleteness and the imperfection of the geological record;" and I want to make it perfectly clear to you that this imperfection is a great fact, which must be taken into account in all our speculations, or we shall constantly be going wrong.

You see the singular series of footmarks, drawn of its natural size in the large diagram hanging up here (Fig. 2), which I owe to the kindness of my friend, Professor Marsh, with whom I had the opportunity recently of visiting the precise locality in Massachusetts in which these tracks

FIG. 2.—TRACKS OF BRONTOZOUM.

occur. I am, therefore, able to give you my own testimony, if needed, that the diagram accurately represents what we saw. The valley of the Connecticut is classical ground for the geologist. It contains great beds of sandstone, covering many square miles, which have evidently formed a part of an ancient sea-shore, or, it may be, lake-shore. For a certain period of time after their deposition, these beds have remained sufficiently soft to receive the impressions of the feet of whatever animals walked over them, and to preserve them afterwards, in exactly the same way as such impressions are at this hour preserved on the shores of the Bay of Funday and elsewhere. The diagram represents the track of some gigantic animal, which walked on its hind legs. You see the series of marks made alternately by the right and by the left foot; so that, from one impression to

the other of the three-toed foot on the same side, is one stride, and that stride, as we measured it, is six feet nine inches. I leave you, therefore, to form an impression of the magnitude of the creature which, as it walked along the ancient shore, made these impressions.

Of such impressions there are untold thousands upon these sandstones. Fifty or sixty different kinds have been discovered, and they cover vast areas. But, up to this present time, not a bone, not a fragment, of any one of the animals which left these great footmarks has been found ; in fact, the only animal remains which have been met with in all these deposits, from the time of their discovery to the present day—though they have been carefully hunted over—is a fragmentary skeleton of one of the smaller forms. What has become of the bones of these animals? You see we are not dealing with little creatures, but with animals that make a step of six feet nine inches; and their remains must have been left somewhere. The probability is, that they have been dissolved away, and absolutely lost.

I have had occasion to work out the nature of fossil remains, of which there was nothing left except casts of the bones, the solid material of the skeleton having been dissolved out by percolating water. It was a chance, in this case, that the sandstone happened to be of such a constitution as to set, and to allow the bones to be afterward dissolved out, leaving cavities of the exact shape of the bones. Had that constitution been other than what it was, the bones would have been dissolved, the layers of sandstone would have fallen together into one mass, and not the slightest indication that the animal had existed would have been discoverable.

I know of no more striking evidence than these facts afford, of the caution which should be used in drawing the conclusion, from the absence of organic remains in a deposit, that animals or plants did not exist at the time it was formed. I believe that, with a right understanding of the doctrine of evolution on the one hand, and a just estimation of the importance of the imperfection of the geological record on the other, all difficulty is removed from the kind of evidence to which I have adverted; and that we are justified in believing that all such cases are examples of what I have designated negative or indifferent evidence—that is to say, they in no way directly advance the hypothesis of evolution, but they are not to be regarded as obstacles in the way of our belief in that doctrine.

I now pass on to the consideration of those cases which, for reasons which I will point out to you by and by, are not to be regarded as demonstrative of the truth of evolution, but which are such as must exist if evolution be true, and which therefore are, upon the whole, evidence in favor of the doctrine. If the doctrine of evolution be true, it follows that, however diverse the different groups of animals and of plants may be, they must all, at one time or other, have been connected by gradational forms; so that, from the highest animals, whatever they may be, down to the lowest speck of protoplasmic matter in which life can be manifested, a series of gradations, leading from one end of the series to the other, either exists or has existed. Undoubtedly that is a necessary postulate of the doctrine of evolution. But when we look upon living Nature as it is, we find a totally different state of things. We find that animals and plants fall into groups, the different members of which are pretty closely allied together, but which are separated by definite, larger or smaller, breaks from other groups. In other words, no intermediate forms which bridge over these gaps or intervals are, at present, to be met with.

To illustrate what I mean: Let me call your attention to those vertebrate animals which are most familiar to you, such as mammals, birds and reptiles. At the present day, these

groups of animals are perfectly well defined from one another. We know of no animal now living which, in any sense, is intermediate between the mammal and the bird, or between the bird and the reptile; but, on the contrary, there are many very distinct anatomical peculiarities, well defined marks, by which the mammal is separated from the bird, and the bird from the reptile. The distinctions are obvious and striking if you compare the definitions of these great groups as they now exist.

The same may be said of many of the subordinate groups, or orders, into which these great classes are divided. At the present time, for example, there are numerous forms of non-ruminant pachyderms, or what we may call broadly, the pig tribe, and many varieties of ruminants. These latter have their definite characteristics, and the former have their distinguishing peculiarities. But there is nothing that fills up the gap between the ruminants and the pig tribe. The two are distinct. Such also is the case in respect of the minor groups of the class of reptiles. The existing fauna shows us crocodiles, lizards, snakes, and tortoises; but no connecting link between the crocodile and lizard, nor between the lizard and the snake, nor between the snake and the crocodile, nor between any two of these groups. They are separated by absolute breaks. If, then, it could be shown that this state of things had always existed, the fact would be fatal to the doctrine of evolution. If the intermediate gradations, which the doctrine of evolution requires to have existed between these groups, are not to be found anywhere in the records of the past history of the globe, their absence is a strong and weighty negative argument against evolution; while, on the other hand, if such intermediate forms are to be found, that is so much to the good of evolution; although, for reasons which I will lay before you by and by, we must be cautious in our estimate of the evidential cogency of facts of this kind.

It is a very remarkable circumstance that, from the commencement of the serious study of fossil remains, in fact, from the time when Cuvier began his brilliant researches upon those found in the quarries of Montmartre, palæontology has shown what she was going to do in this matter, and what kind of evidence it lay in her power to produce.

I said just now that, in the existing Fauna, the group of pig-like animals and the group of ruminants are entirely distinct; but one of the first of Cuvier's discoveries was an animal which he called the *Anoplotherium*, and which proved to be, in a great many important respects, intermediate in character between the pigs, on the one hand, and the ruminants on the other. Thus research into the history of the past did, to a certain extent, tend to fill up the breach between the group of ruminants and the group of pigs. Another remarkable animal restored by the great French palæontologist, the *Palæotherium*, similarly tended to connect together animals to all appearance so different as the rhinoceros, the horse, and the tapir. Subsequent research has brought to light multitudes of facts of the same order; and, at the present day, the investigations of such anatomists as Rutimeyer and Gaudry have tended to fill up, more and more, the gaps in our existing series of mammals, and to connect groups formerly thought to be distinct.

But I think it may have an especial interest if, instead of dealing with these examples, which would require a great deal of tedious osteological detail, I take the case of birds and reptiles; groups which, at the present day, are so clearly distinguished from one another that there are perhaps no classes of animals which, in popular apprehension, are more completely separated. Existing birds, as you are aware, are covered with feathers; their anterior extremeties, specially and peculiarly modified, are converted into wings, by the aid of which most of them are able to fly; they walk upright on two legs; and these limbs,

when they are considered anatomically, present a great number of exceedingly remarkable peculiarities, to which I may have occasion to advert incidentally as I go on, and which are not met with, even approximately, in any existing forms of reptiles. On the other hand, existing reptiles have no feathers. They may have naked skins, or be covered with horny scales, or bony plates, or with both. They possess no wings; they neither fly by means of their fore-limbs, nor habitually walk upright upon their hind-limbs; and the bones of their legs present no such modifications as we find in birds. It is impossible to imagine any two groups more definitely and distinctly separated, notwithstanding certain characters which they possess in common.

As we trace the history of birds back in time, we find their remains, sometimes in great abundance, throughout the whole extent of the tertiary rocks; but, so far as our present knowledge goes, the birds of the tertiary rocks retain the same essential characters as the birds of the present day. In other words, the tertiary birds come within the definition of the class constituted by existing birds, and are as much separated from reptiles as existing birds are.

Not very long ago no remains of birds had been found below the tertiary rocks, and I am not sure but that some persons were prepared to demonstrate that they could not have existed at an earlier period. But, in the course of the last few years, such remains have been discovered in England; though, unfortunately, in so imperfect and fragmentary a condition, that it is impossible to say whether they differed from existing birds in any essential character or not. In your country, the development of the cretaceous series of rocks is enormous; the conditions under which the latter cretaceous strata have been deposited are highly favorable to the preservation of organic remains; and the researches, full of labor and risk, which have been carried on by Professor Marsh in these cretaceous rocks of Western America, have rewarded him with the discovery of forms of birds of which we had hitherto no conception. By his kindness, I am enabled to place before you a restoration of one of these extraordinary birds, every part of which can be thoroughly justified by the more or less complete skeletons, in a perfect state of preservation, which he has discovered. This *Hesperornis* (Fig 3), which measured between five and six feet in length, is astonishingly like our existing divers or grebes in a great many respects; so like them, indeed, that, had the skeleton of *Hesperornis* been found in a museum without its skull, it probably would have been placed in the same group of birds as the divers and grebes of the present day.* But *Hesperornis* differs from all existing birds, and so far resembles reptiles, in one important particular—it is provided with teeth. The long jaws are armed with teeth which have curved crowns and thick roots (Fig. 4), and are not set in distinct sockets, but are lodged in a groove. In possessing true teeth, the *Hesperornis* differs from every existing bird, and from every bird yet discovered in the tertiary formations, the tooth-like serrations of the jaws in the *Odontopteryx* of the London clay, being mere processes of the bony substance of the jaws, and not teeth in the proper sense of the word. In view of the characteristics of this bird we are therefore obliged to modify the definitions of the classes of birds and reptiles. Before the discovery of *Hesperornis*, the definition of the class Aves based upon our knowledge of existing birds, might have been extended to all birds; it might have been said that the absence of teeth was characteristic of

* The absence of any keel on the breast-bone and some other osteological peculiarities, observed by Professor Marsh, however, suggest that *Hesperornis* may be a modification of a less specialized group of birds than that to which these existing acquatic birds belong.

the class of birds; but the discovery of an animal which, in every part of its skeleton, closely agrees with existing birds, and yet possesses teeth, shows that there were ancient birds which in respect of possessing teeth, approached reptiles more nearly than any existing bird does, and, to that extent, diminishes the *hiatus* between the two classes.

The same formation has yielded another Bird *Ichthyornis* (Fig. 5), which also possesses teeth; but the teeth are situated in distinct sockets, while those of *Hesperornis* are not so lodged. The latter also has such very small, almost rudimentary, wings, that it must have been chiefly a swimmer and a diver, like a Penguin; while *Ichthyornis* has strong wings, and no doubt possessed corresponding powers of flight. *Ichthyornis* also differed in the fact that its vertebræ have not the peculiar characters of the vertebræ of existing and of all known tertiary birds, but were concave at each end. This discovery leads us to make a further modification in the definition of the group of birds, and to part with another of the characters by which almost all existing birds are distinguished from reptiles.

Apart from the few fragmentary remains from the English greensand, to which I have referred, the mesozoic rocks, older than those in which *Hesperornis* and *Ichthyornis* have been discovered have afforded no certain evidence of birds, with the remarkable exception of the Solenhofen slates. These so-called slates are composed of a fine grained calcareous mud, which has hardened into lithographic stone, and in which organic remains are almost as well preserved as they would be if they had been imbedded in so much plaster of Paris. They yielded the *Archæ-*

Fig. 3.—Hesperornis Regalis (Marsh).

opteryx, the existence of which was first made known by the finding of a fossil feather, or rather of the impression of one. It is wonderful enough that such a perishable thing as a feather, and nothing more, should fortunately wanting, and it is therefore uncertain whether the *Archæopteryx* possessed teeth or not. But the remainder of the skeleton is so well preserved as to leave no doubt respecting the main features of the

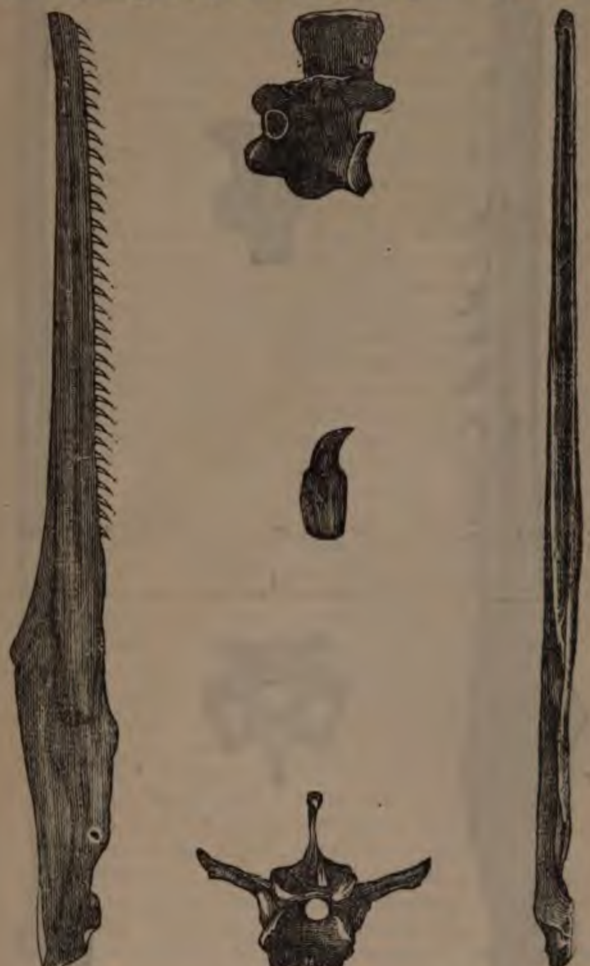


FIG. 4.—HESPERORNIS REGALIS (Marsh).

(Side and upper views of half the lower jaw; side and end views of a vertebra and a separate tooth.)

be discovered; yet, for a long time, nothing was known of this bird except its feather. But, by and by a solitary skeleton was discovered, which is now in the British Museum. The skull of this solitary specimen is un- animal, which are very singular. The feet are not only altogether bird-like, but have the special characters of the feet of perching birds, while the body had a clothing of true feathers. Nevertheless, in some other

respects, *Archæopteryx* is unlike a bird and like a reptile. There is a long tail composed of many vertebræ. The structure of the wing differs in some very remarkable respects to the bones of the fingers which lie in the palm of the hand, are fused together into one mass; and the whole apparatus, except the last joints of the thumb, is bound up in a sheath of

FIG. 5.—ICHTHYORNIS DISPAR (Marsh.)

(Side and upper views of half the lower jaw; and side and end views of a vertebra.)

from that which it presents in a true bird. In the latter, the end of the wing answers to the thumb and two fingers of my hand; but the metacarpal bones, or those which answer integument, while the edge of the hand carries the principal quill-feathers. In the *Archæopteryx*, the upper-arm bone is like that of a bird; and the two bones of the forearm are

more or less like those of a bird, but the fingers are not bound together—they are free. What their number may have been is uncertain; but several, if not all, of them were terminated by strong curved claws, not like such as are sometimes found in birds, but such as reptiles possess; so that, in the *Archæopteryx*, we have an animal which, to a certain extent, occupies a midway place between a bird and a reptile. It is a bird so far as its foot and sundry other parts of its skeleton are concerned; it is essentially and thoroughly a bird by its feathers; but it is much more properly a reptile in the fact that the region which represents the hand has separate bones, with claws resembling those which terminate the fore limb of a reptile. Moreover, it had a long reptile like tail with a fringe of feathers on each side; while in all true birds hitherto known, the tail is relatively short, and the vertebræ which constitute its skeleton are generally peculiarly modified.

Like the *Anoplotherium* and the *Palæotherium*, therefore, *Archæopteryx* tends to fill up the interval between groups which, in the existing world, are widely separated, and to destroy the value of the definitions of zoological groups based upon our knowledge of existing forms. And such cases as these constitute evidence in favor of evolution, in so far as they prove that, in former periods of the world's history, there were animals which overstepped the bounds of existing groups, and tended to merge them into larger assemblages. They show that animal organization is more flexible than our knowledge of recent forms might have led us to believe; and that many structural permutations and combinations, of which the present world gives us no indication, may nevertheless have existed.

But it by no means follows, because the *Palæotherium* has much in common with the Horse, on the one hand, and with the Rhinoceros on the other, that it is the intermediate form through which Rhinoceroses have passed to become Horses, or *vice versa*; on the contrary, any such supposition would certainly be erroneous. Nor do I think it likely that the transition from the reptile to the bird has been effected by such a form as *Archæopteryx*. And it is convenient to distinguish these intermediate forms between two groups, which do not represent the actual passage from the one group to the other, as *intercalary* types, from those *linear* types which, more or less approximately, indicate the nature of the steps by which the transition from one group to the other was effected.

I conceive that such linear forms, constituting a series of natural gradations between the reptile and the bird, and enabling us to understand the manner in which the reptilian has been metamorphosed into the bird type, are really to be found among a group of ancient and extinct terrestrial reptiles known as the *Ornithoscelida*. The remains of these animals occur througout the series of mesozoic formations, from the Trias to the Chalk, and there are indications of their existence even in the latter Palæozoic strata.

Most of these reptiles at present known are of great size, some having attained a length of forty feet or perhaps more. The majority resembled lizards and crocodiles in their general form, and many of them were, like crocodiles, protected by an armor of heavy bony plates. But, in others, the hind limbs elongate and the fore limbs shorten, until their relative proportions approach those which are observed in the short-winged, flightless, ostrich tribe among birds.

The skull is relatively light, and in some cases the jaws, though bearing teeth, are beak-like at their extremities and appear to have been enveloped in a horny sheath. In the part of the vertebral column which lies between the haunch bones and is called the sacrum, a number of vertebræ may unite together into one whole, and in this respect as in some

details of its structure, the sacrem of these reptiles approaches that of birds.

But it is in the structure of the pelvis and of the hind limb that some of these ancient reptiles present the most remarkable approximation to birds, and clearly indicate the way by which the most specialized and characteristic features of the bi d may have been evolved from the corresponding parts in the reptile.

In Fig. 6, the pelvis and hind limbs of a crocodile, a three-toed bird, and the ground. Hence, in the crocodile, the body usually lies squat between the legs, while, in the bird, it is raised upon the hind legs, as upon pillars.

In the crocodile, the pelvis is obviously composed of three bones on each side: the ilium (*Il.*), the pubis (*Pb.*), and the ischium (*Is.*). In the adult bird there appears to be but one bone on each side. The examination of the pelvis of a chick, however, shows that each half is made up of three bones, which answer to those which remain distinct throughout life,

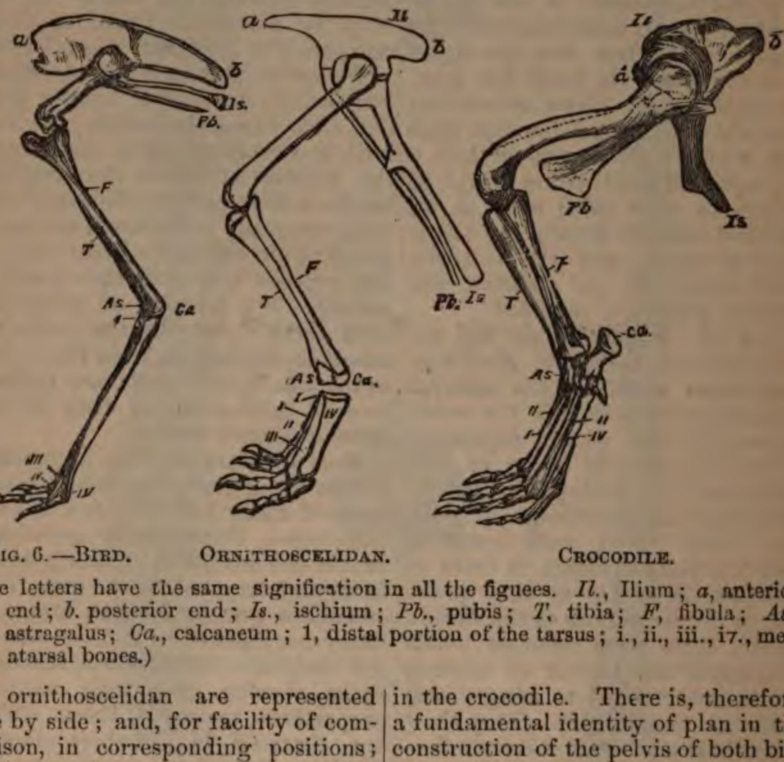


FIG. 6.—BIRD. ORNITHOSCELIDAN. CROCODILE.

(The letters have the same signification in all the figuees. *Il.*, Ilium; *a*, anterior end; *b.* posterior end; *Is.*, ischium; *Pb.*, pubis; *T*, tibia; *F*, fibula; *As.*, astragalus; *Ca.*, calcaneum; 1, distal portion of the tarsus; i., ii., iii., i7., metatarsal bones.)

an ornithoscelidan are represented side by side; and, for facility of comparison, in corresponding positions; but it must be recollected that, while the position of the bird's limb is natural, that of the crocodile is not so. In the bird, the thigh-bone lies close to the body, and the metatarsal bones of the foot (ii., iii., iv., Fig. 6) are, ordinarily, raised into a more or less vertical position; in the crocodile, the thigh-bone stands out at an angle from the body, and the metatarsal bones (i., ii., iii., iv., Fig. 6) lie flat on in the crocodile. There is, therefore, a fundamental identity of plan in the construction of the pelvis of both bird and reptile; though the differences in form, relative size, and direction of the corresponding bones in the two cases are very great.

But the most striking contrast between the two lies in the bones of the leg and of that part of the foot termed the tarsus, which follows upon the leg. In the crocodile, the fibula (*E*) is relatively large and its lower end is complete. The tibia (*T*) has no

marked crest at its upper end, and its lower end is narrow and not pulley-shaped. There are two rows of separate tarsal bones (*As.*, *Ca.*, *&c.*) and four distinct metatarsal bones, with a rudiment of a fifth.

In the bird, the fibula is small and its lower end diminishes to a point. The tibia has a strong crest at its upper end and its lower extremity passes into a broad pulley. There seem at first to be no tarsal bones; and only one bone, divided at the end into three heads for the three toes which are attached to it, appears in the place of the metatarsus.

In a young bird, however, the pulley shaped apparent end of the tibia is a distinct bone, which represents the bones marked *As.*, *Ca.*, in the crocodile; while the apparently single metatarsal bone consists of three bones, which early unite with one another and with an additional bone, which represents the lower row of bones in the tarsus of the crocodile.

In other words, it can be shown by the study of developement that the bird's pelvis and hind limb are simply extreme modifications of the same fundamental plan as that upon which these parts are modelled in reptiles.

On comparing the pelvis and hind limb of the ornithoscelidan with that of the crocodile, on the one side, and that of the bird, on the other (Fig 6), it is obvious that it represents a middle term between the two. The pelvic bones approach the form of those of the birds, and the direction of the pubis and ischium is nearly that which is characteristic of birds; the thigh bone, from the direction of its head, must have lain close to the body; the tibia has a great crest, and, immovably fitted on to its lower end, there is a pulley-shaped bone, like that of the bird, but remaining distinct. The lower end of the fibula is much more slender, proportionally, than in the crocodile. The metatarsal bones have such a form that they fit together immovably, though they do not enter into bony union; the third toe is, as in the bird, longest and strongest. In fact, the ornithoscelidan limb is comparable to that of an unhatched chick.

Taking all these facts together, it is obvious that the view which was entertained by Mantell and the probability of which was demonstrated by your own distinguished anatomist, Leidy, while much additional evidence in the same direction has been furnished by Professor Cope, that some of these animals may have walked upon their hind legs, as birds do, acquires great weight. In fact, there can be no reasonable doubt that one of the smaller forms of the *Ornithoscelida*, *Compsognathus*, the almost entire skeleton of which has been discovered in the Solenhofen slates, was a bipedal animal. The parts of this skeleton are somewhat twisted out of their natural relations,

FIG. 7.—RESTORATION OF COMPSOGNATHUS LONGIPES.

but the accompanying figure gives a just view of the general form of *Compsognathus* and of the proportion of its limbs; which, in some respects, are more completely bird like than those of other *Ornithoscelida*.

We have had to stretch the definition of the class of birds so as to include birds with teeth and birds with paw-like fore-limbs and long tails. There is no evidence that *Compsogn-*

athus possessed feathers; but, if it did, it would be hard indeed to say whether it should be called a reptilian bird or an avian reptile.

As *Compsognathus* walked upon his hind legs, it must have made tracks like those of birds. And as the structure of the limbs of several of the gigantic *Ornithoscelida*, such as *Iguandon*, leads to the conclusion that they also may have constantly, or occasionally, assumed the same attitude, a peculiar interest attaches to the fact that, in the Wealden strata of England, there are to be found gigantic footsteps, arranged in order like those of the *Brontozoum*, and which there can be no reasonable doubt were made by some of the *Ornithoscelida*, the remains of which are found in the same rocks. And, knowing that reptiles that walked upon their hind legs and shared many of the anatomical characters of birds did once exist, it becomes a very important question whether the tracks in the Trias of Massachusetts, to which I referred some time ago, and which formerly used to be unhesitatingly ascribed to birds, may not all have been made by Ornithoscelidan reptiles; and whether, if we could obtain the skeletons of the animals which made these tracks, we should not find in them the actual steps of the evolutional process by which reptiles gave rise to birds.

The evidential value of the facts I have brought forward in this Lecture must be neither over nor under estimated. It is not historical proof of the occurrence of the evolution of birds from reptiles, for we have no safe ground for assuming that true birds had not made their appearance at the commencement of the Mesozoic epoch. It is, in fact, quite possible that all these more or less avi-form reptiles of the Mesozoic epoch are not terms in the series of progression from birds to reptiles at all, but simply the more or less modified descendants of Palæozoic forms through which that transition was actually effected.

We are not in a position to say that the known *Ornithoscelida* are intermediate in the order of their appearance on the earth between reptiles and birds. All that can be said is that, if independent evidence of the actual occurrence of evolution is producible, then these intercalary forms remove every difficulty in the way of understanding what the actual steps of the process, in the case of birds, may have been.

That intercalary forms should have existed in ancient times is a necessary consequence of the truth of the hypothesis of evolution; and, hence, the evidence I have laid before you in proof of the existence of such forms, is, so far as it goes, in favor of that hypothesis.

There is another series of extinct reptiles, which may be said to be intercalary between reptiles and birds, in so far as they combine some of the characters of both these groups; and, which, as they possessed the power of flight, may seem, at first sight, to be nearer representatives of the forms by which the transition from the reptile to the bird was effected, than the *Ornithoscelida*.

These are the *Pterosauria*, or Pterodactyles, the remains of which are met with throughout the series of Mesozoic rocks, from the lias to the chalk, and some of which attained a great size, their wings having a span of eighteen or twenty feet. These animals, in the form and proportions of the head and neck relatively to the body, and in the fact that the ends of the jaws were often, if not always, more or less extensively ensheathed in horny beaks, remind us of birds. Moreover, their bones contained air cavities, rendering them specifically lighter, as is the case in most birds. The breast bone was large and keeled, as in most birds and in bats, and the shoulder girdle is strikingly similar to that of ordinary birds. But, it seems to me, that the special resemblance of pterodactyles to birds ends here, unless I may add the entire absence of teeth which

characterizes the great pterodactyles (*Pteranodon*) discovered by Professor Marsh. All other known pterodactyles have teeth lodged in sockets. In the vertebral column and the hind limbs there are no special resemblances to birds, and when we turn to the wings they are found to be constructed on a totally different principle from those of birds.

There are four fingers. These four fingers are large, and three of them, those which answer to the thumb and and two following fingers of my hand — are terminated by claws, while the fourth is enormously prolonged and converted into a great joint style. You see at once, from what I have stated about a bird's wing, that there could be nothing less like a bird's wing than this is. It was concluded by general reasoning that this finger had the office of supporting a web which extended between it and the body. An existing specimen proves that such was really the case, and that the pterodactyles were devoid of feathers, but that the fingers supported a vast web like that of a bat's wing; in fact, there can be no doubt that this ancient reptile flew after the fashion of a bat.

Thus though the pterodactyle is a reptile which has become modified in such a manner as to enable it to fly, and therefore, as might be expected, presents some points of resemblance to other animals which fly; it has, so to speak, gone off the line which leads directly from reptiles to birds, and has become disqualified for the changes which lead to the characteristic organization of the latter class. Therefore, viewed in relation to the classes of reptiles and birds, the pterodactyles appear to me to be, in a

FIG. 8.—PTERODACTYLUS SPECTABILIS (Von Meyer).

limited sense, intercalary forms; but they are not even approximately linear, in the sense of exemplifying these modifications of structure through which the passage from the reptile to the bird took place.

LECTURE III.

The Demonstrative Evidence of Evolution.

THE occurrence of historical facts is said to be demonstrated, when the evidence that they happened is of such a character as to render the assumption that they did not happen in the highest degree improbable; and the question I now have to deal with is, whether evidence in favor of the evolution of animals of this degree of cogency is, or is not, obtainable from the record of the succession of living forms which is presented to us by fossil remains.

Those who have attended to the progress of palæontology are aware that evidence of the character which I have defined has been produced in considerable and continually-increasing quantity during the last few years. Indeed, the amount and the satisfactory nature of that evidence are somewhat surprising, when we consider the conditions under which alone we can hope to obtain it.

It is obviously useless to seek for such evidence, except in localities in which the physical conditions have been such as to permit of the deposit of an unbroken, or but rarely interrupted, series of strata through a long period of time; in which the group of animals to be investigated has existed in such abundance as to furnish the requisite supply of remains; and in which, finally, the materials composing the strata are such as to ensure the preservation of these remains in a tolerably perfect and undisturbed state.

It so happens that the case which, at present, most nearly fulfils all these conditions is that of the series of extinct animals which culminates in the Horses; by which term I mean to denote not merely the domestic animals with which we are all so well acquainted, but their allies, the ass, zebra, quagga, and the like. In short, I use "horses" as the equivalent of the technical name *Equidæ*, which is applied to the whole group of existing equine animals.

The horse is in many ways a remarkable animal; not least so in the fact that it presents us with an example of one of the most perfect pieces of machinery in the living world. In truth, among the works of human ingenuity it cannot be said that there is any locomotive so perfectly adapted to its purposes, doing so much work with so small a quantity of fuel, as this machine of nature's manufacture—the horse. And, as a necessary consequence of any sort of perfection, of mechanical perfection as of others, you find that the horse is a beautiful creature, one of the most beautiful of all land-animals. Look at the perfect balance of his form, and the rhythm and force of its action. The locomotive machinery is, as you are aware, resident in its slender fore and hind limbs; they are flexible and elastic levers, capable of being moved by very powerful muscles; and, in order to supply the engines which work these levers with the force which they expend, the horse is provided with a very perfect apparatus for grinding its food and extracting therefrom the requisite fuel.

Without attempting to take you very far into the region of osteological detail, I must nevertheless trouble you with some statements respecting the anatomical structure of the horse; and, more especially, will it be needful to obtain a general conception of the structure of its fore and hind limbs, and of its teeth. But I shall only touch upon those points which are absolutely essential to our inquiry.

Let us turn in the first place to the fore-limb. In most quadrupeds, as in ourselves, the fore-arm contains distinct bones called the radius and the ulna. The corresponding region in

the horse seem at first to possess but one bone. Careful observation, however, enables us to distinguish in this bone a part which clearly answers to the upper end of the ulna. This is closely united with the chief mass of the bone which represents the radius, and runs out into a slender shaft which may be traced for some distance downwards upon the back of the radius, and then in most cases thins out and vanishes. It takes still more trouble to make sure of what is nevertheless the fact, that a small part of the lower end of the bone of a horse's fore-arm, which is only distinct in a very young foal, is really the lower extremity of the ulna.

What is commonly called the knee of a horse is its wrist. The "cannon bone" answers to the middle bone of the five metacarpal bones, which support the palm of the hand in ourselves. The "pastern," "coronary," and "coffin" bones of veterinarians answer to the joints of our middle fingers, while the hoof is simply a greatly enlarged and thickened nail. But if what lies below the horse's "knee" thus corresponds to the middle finger in ourselves, what has become of the four other fingers or digits? We find in the places of the second and fourth digits only two slender splint-like bones, about two-thirds as long as the cannon bone, which gradually taper to their lower ends and bear no finger joints, or, as they are termed, phalanges. Sometimes, small bony or gristly nodules are to be found at the bases of these two metacarpal splints, and it is probable that these represent rudiments of the first and fifth toes. Thus, the part of the horse's skeleton which corresponds with that of the human hand, contains one overgrown middle digit, and at least two imperfect lateral digits; and these answer, respectively, to the third, the second, and the fourth fingers in man.

Corresponding modifications are found in the hind limb. In ourselves, and in most quadrupeds, the leg contains two distinct bones, a large bone, the tibia, and a smaller and more slender bone, the fibula. But, in the horse, the fibula seems, at first, to be reduced to its upper end; a short slender bone united with the tibia, and ending in a point below, occupying its place. Examination of the lower end of a young foal's shinbone, however, shows a distinct portion of osseous matter which is the lower end of the fibula; so that the, apparently single, lower end of the shin-bone is really made up of the coalesced ends of the tibia and fibula, just as the, apparently single, lower end of the fore-arm bone is composed of the coalesced radius and ulna.

The heel of the horse is the part commonly known as the hock. The hinder cannon bone answers to the middle metatarsal bone of the human foot, the pastern, coronary, and coffin bones, to the middle toe bones; the hind hoof to the nail; as in the fore-foot. And, as in the fore-foot, there are merely two splints to represent the second and the fourth toes. Sometimes a rudiment of a fifth toe appears to be traceable.

The teeth of a horse are not less peculiar than its limbs. The living engine, like all others, must be well stoked if it is to do its work; and the horse, if it is to make good its wear and tear, and to exert the enormous amount of force required for its propulsion, must be well and rapidly fed. To this end, good cutting instruments and powerful and lasting crushers are needful. Accordingly, the twelve cutting teeth of a horse are close-set and concentrated in the fore part of its mouth, like so many adzes or chisels. The grinders or molars are large, and have an extremely complicated structure, being composed of a number of different substances of unequal hardness. The consequence of this is that they wear away at different rates; and, hence, the surface of each grindler is always as uneven as that of a good millstone.

I have said that the structure of the grinding teeth is very complicated, the harder and the softer parts being, as it were, interlaced with one another.

The result of this is that, as the tooth wears, the crown presents a peculiar pattern, the nature of which is not very easily deciphered at first, but which it is important that we should understand clearly. Each grinding tooth of the upper jaw has an *outer wall* so shaped that, on the worn crown, it exhibits the form of two crescents, one in front and one behind, with their concave sides turned outwards. From the inner sides of the front crescent, a crescentic *front ridge* passes inwards and backwards, and its inner face enlarges into a strong longitudinal fold or *pillar*. From the front part of the hinder crescent, a *back ridge* takes a like direction, and also has its *pillar*.

The deep interspaces or *valleys* between these ridges and the outer wall are filled by bony substance, which is called *cement*, and coats the whole tooth.

The pattern of the worn face of each grinding tooth of the lower jaw is quite different. It appears to be formed of two crescent-shaped ridges, the convexities of which are turned outwards. The free extremity of each crescent has a *pillar*, and there is a large double *pillar* where the two crescents meet. The whole structure, is, as it were, imbedded in cement, which fills up the valleys, as in the upper grinders.

If the grinding faces of an upper and of a lower molar of the same side are applied together, it will be seen that the apposed ridges are nowhere parallel, but that they frequently cross; and that thus, in the act of mastication, a hard surface in the one is constantly applied to a soft surface in the other, and *vice versa*. They thus constitute a grinding apparatus of great efficiency, and one which is repaired as fast as it wears, owing to the long continued growth of the teeth.

Some other peculiarities of the dentition of the horse must be noticed, as they bear upon what I shall have to say by and by. Thus the crowns of the cutting teeth have a peculiar deep pit, which gives rise to the well-known "mark" of the horse. There is a large space between the outer incisors and the front grinder. In this space the adult male horse presents, near the incisors one on each side, above and below, a canine or "tush," which is commonly absent in mares. In a young horse, moreover, there is not unfrequently to be seen, in front of the first grinder, a very small tooth, which soon falls out. If this small tooth be counted as one, it will be found that there are seven teeth behind the canine on each side; namely, the small tooth in question, and the six great grinders, among which, by an unusual peculiarity, the foremost tooth is rather larger than those which follow it.

I have now enumerated those characteristic structures of the horse, which are of most importance for the purpose we have in view.

To any one who is acquainted with the morphology of vertebrated animals, they show that the horse deviates widely from the general structure of mammals; and that the horse type is, in many respects, an extreme modification of the general mammalian plan. The least modified mammals, in fact, have the radius and ulna, the tibia and fibula, distinct and separate. They have five distinct and complete digits on each foot, and no one of these digits is very much larger than the rest. Moreover, in the least modified mammals, the total number of the teeth is very generally forty-four, while in horses, the usual number is forty, and in the absence of the canines, it may be reduced to thirty-six; the incisor teeth are devoid of the fold seen in those of the horse; the grinders regularly diminish in size from the middle of the series to its front end; while their crowns are short, early attain their full length, and exhibit simple ridges or tubercles, in place of the complex foldings of the horse's grinders.

Hence the general principles of the hypothesis of evolution lead to the conclusion that the horse must have

been derived from some quadruped which possessed five complete digits on each foot; which had the bones of the fore-arm and of the leg complete and separate; and which possessed forty-four teeth, among which the crowns of the incisors and grinders had a simple structure; while the latter gradually increased in size from before backwards, at any rate in the anterior part of the series, and had short crowns.

And if the horse has been thus evolved, and the remains of the different stages of its evolution have been preserved, they ought to present us with a series of forms in which the number of the digits becomes reduced; the bones of the fore-arm and leg gradually take on the equine condition; and the form and arrangement of the teeth successively approximate to those which obtain in existing horses.

Let us turn to the facts, and see how far they fulfil these requirements of the doctrine of evolution.

In Europe abundant remains of horses are found in the Quaternary and later Tertiary strata as far as the Pliocene formation. But these horses, which are so common in the cave-deposits and in the gravels of Europe, are in all essential respects like existing horses. And that is true of all the horses of the latter part of the Pliocene epoch. But, in deposits which belong to the earlier Pliocene and later Miocene epochs, and which occur in Britain, in France, in Germany, in Greece, in India, we find animals which are extremely like horses—which, in fact, are so similar to horses, that you may follow descriptions given in works upon the anatomy of the horse upon the skeletons of these animals—but which differ in some important particulars. For example, the structure of their fore and hind limbs is somewhat different. The bones which, in the horse, are represented by two splints, imperfect below, are as long as the metacarpal and metatarsal bones; and, attached to the extremity of each, is a digit with three joints of the same general character as those of the middle digit, only very much smaller. These small digits are so disposed that they could have had but very little functional importance, and they must have been rather of the nature of the dew-claws, such as are to be found in many ruminant animals. The *Hipparion*, as the extinct European three-toed horse is called, in fact, presents a foot similar to that of the American *Protohippus* (Fig. 9), except that, in the *Hipparion*, the smaller digits are situated farther back, and are of smaller proportional size, than in the *Protohippus*.

The ulna is slightly more distinct than in the horse; and the whole length of it, as a very slender shaft, intimately united with the radius, is completely traceable. The fibula appears to be in the same condition as in the horse. The teeth of the *Hipparion* are essentially similar to those of the horse, but the pattern of the grinders is in some respects a little more complex, and there is a depression on the face of the skull in front of the orbit, which is not seen in existing horses.

In the earlier Miocene, and perhaps the later Eocene deposits of some parts of Europe, another extinct animal has been discovered, which Cuvier, who first described some fragments of it, considered to be a *Palæotherium*. But as further discoveries threw no light upon its structure, it was recognized as a distinct genus, under the name of *Anchitherium*.

In its general characters, the skeleton of *Anchitherium* is very similar to that of the horse. In fact, Lartet and De Blainville called it *Palæotherium equinum* or *hippoides*; and De Christol, in 1847, said that it differed from *Hipparion* in little more than the characters of its teeth, and gave it the name of *Hipparitherium*. Each foot possesses three complete toes; while the lateral toes are much larger in proportion to the

middle toe than in *Hipparion*, and doubtless rested on the ground in ordinary locomotion.

The ulna is complete and quite distinct from the radius, though firmly united with the latter. The fibula seems also to have been complete. Its lower end, though intimately united with that of the tibia, is clearly marked off from the latter bone.

There are forty-four teeth. The incisors have no strong pit. The canines seem to have been well developed in both sexes. The first of the seven grinders, which, as I have said, is frequently absent, and, when it does exist, is small in the horse, is a good sized and permanent tooth, while the grinder which follows it is but little larger than the hinder ones. The crowns of the grinders are short, and though the fundamental pattern of the horse-tooth is discernible, the front and back ridges are less curved, the accessory pillars are wanting, and the valleys, much shallower, are not filled up with cement.

Seven years ago, when I happened to be looking critically into the bearing of palæontological facts upon the doctrine of evolution, it appeared to me that the *Anchitherium*, the *Hipparion*, and the modern horses, constitute a series in which the modifications of structure coincide with the order of chronological occurrence, in the manner in which they must coincide, if the modern horses really are the result of the gradual metamorphosis, in the course of the Tertiary epoch, of a less specialized ancestral form. And I found by correspondence with the late eminent French anatomist and palæontologist, M. Lartet, that he had arrived at the same conclusion from the same data.

That the *Anchitherium* type had become metamorphosed into the *Hipparion* type, and the latter into the *Equine* type, in the course of that period of time which is represented by the latter half of the Tertiary deposits, seemed to me to be the only explanation of the facts for which there was even a shadow of probability.*

And, hence, I have ever since held that these facts afford evidence of the occurrence of evolution, which, in the sense already defined, may be termed demonstrative.

All who have occupied themselves with the structure of *Anchitherium*, from Cuvier onwards, have acknowledged its many points of likeness to a well-known genus of extinct Eocene mammals, *Palæotherium*. Indeed, as we have seen, Cuvier regarded his remains of *Anchitherium* as those of a species of *Palæotherium*. Hence, in attempting to trace the pedigree of the horse beyond the Miocene epoch and the Anchitheroid form, I naturally sought among the various species of Palæotheroid animals for its nearest ally, and I was led to conclude that the *Palæotherium minus* (*Plagiolophus*) represented the next step more nearly than any form then known.

I think that this opinion was fully justifiable; but the progress of investigation has thrown an unexpected light on the question, and has brought us much nearer than could have been anticipated to a knowledge of the true series of the progenitors of the horse.

You are all aware that when your country was first discovered by Europeans, there were no traces of the existence of the horse in any part of the American Continent. The accounts of the conquest of Mexico dwell upon the astonishment of the natives of that country when they first became acquainted with that astounding phenomenon — a man

* I use the word "type" because it is highly probable that many forms of *Anchitherium*-like and *Hipparion*-like animals existed in the Miocene and Pliocene epochs, just as many species of the horse tribe exist now; and it is highly improbable that the particular species of *Anchitherium* or *Hipparion*, which happen to have been discovered, should be precisely those which have formed part of the direct line of the horse's pedigree.

seated upon a horse. Nevertheless, the investigations of American geologists have proved that the remains of horses occur in the most superficial deposits of both North and South America, just as they do in Europe. Therefore, for some reason or other no feasible suggestion on that subject so far as I know has been made—the horse must have died out in this continent at some period preceding the discovery of America. Of late years there has been discovered in your Western Territories that marvellous accumulation of deposits, admirably adapted for the preservation of organic remains, to which I referred the other evening, and which furnishes us with a consecutive series of records of the fauna of the older half of the Tertiary epoch, for which we have no parallel in Europe. They have yielded fossils in an excellent state of conservation and in unexampled number and variety. The researches of Leidy and others have shown that forms allied to the *Hipparion* and the *Anchitherium* are to be found among these remains. But it is only recently that the admirably conceived and most thoroughly and patiently worked-out investigations of Professor Marsh have given us a just idea of the vast fossil wealth, and of the scientific importance of these deposits. I have had the advantage of glancing over the collections in Yale Museum, and I can truly say that, so far as my knowledge extends, there is no collection from any one region and series of strata comparable for extent, or for the care with which the remains have been got together, or for their scientific importance, to the series of fossils which he has deposited there. This vast collection has yielded evidence bearing upon the question of the pedigree of the horse of the most striking character. It tends to show that we must look to America, rather than to Europe, for the original seat of the equine series; and that the archaic forms and successive modifications of the horse's ancestry are far better preserved here than in Europe.

Professor Marsh's kindness has enabled me to put before you a diagram, every figure in which is an actual representation of some specimen which is to be seen at Yale at this present time (Fig. 9).

The succession of forms which he has brought together carries us from the top to the bottom of the Tertiaries. Firstly, there is the true horse. Next we have the American Pliocene form of the horse (*Pliohippus*); in the conformation of its limbs it presents some very slight deviations from the ordinary horse, and the crowns of the grinding teeth are shorter. Then comes the *Protohippus*, which represents the European *Hipparion*, having one large digit and two small ones on each foot, and the general characters of the fore-arm and leg to which I have referred. But it is more valuable than the European *Hipparion* for the reason that it is devoid of some of the peculiarities of that form—peculiarities which tend to show that the European *Hipparion* is rather a member of a collateral branch than a form in the direct line of succession. Next, in the backward order in time, is the *Miohippus*, which corresponds pretty nearly with the *Anchitherium* of Europe. It presents three complete toes—one large median and two smaller lateral ones; and there is a rudiment of that digit, which answers to the little finger of the human hand.

The European record of the pedigree of the horse stops here; in the American Tertiaries, on the contrary, the series of ancestral equine forms is continued into the Eocene formations. An older Miocene form, termed *Mesohippus*, has three toes in front, with a large splint-like rudiment representing the little finger; and three toes behind. The radius and ulna, the tibia and the fibula, are distinct, and the short-crowned molar teeth are anchitheroid in pattern.

But the most important discovery of all is the *Orohippus*, which comes from the Eocene formation, and is the oldest member of the equine series, as yet known. Here we find four com-

plete toes on the front-limb, three toes on the hind-limb, a well-developed ulna, a well-developed fibula, and short-crowned grinders of simple pattern.

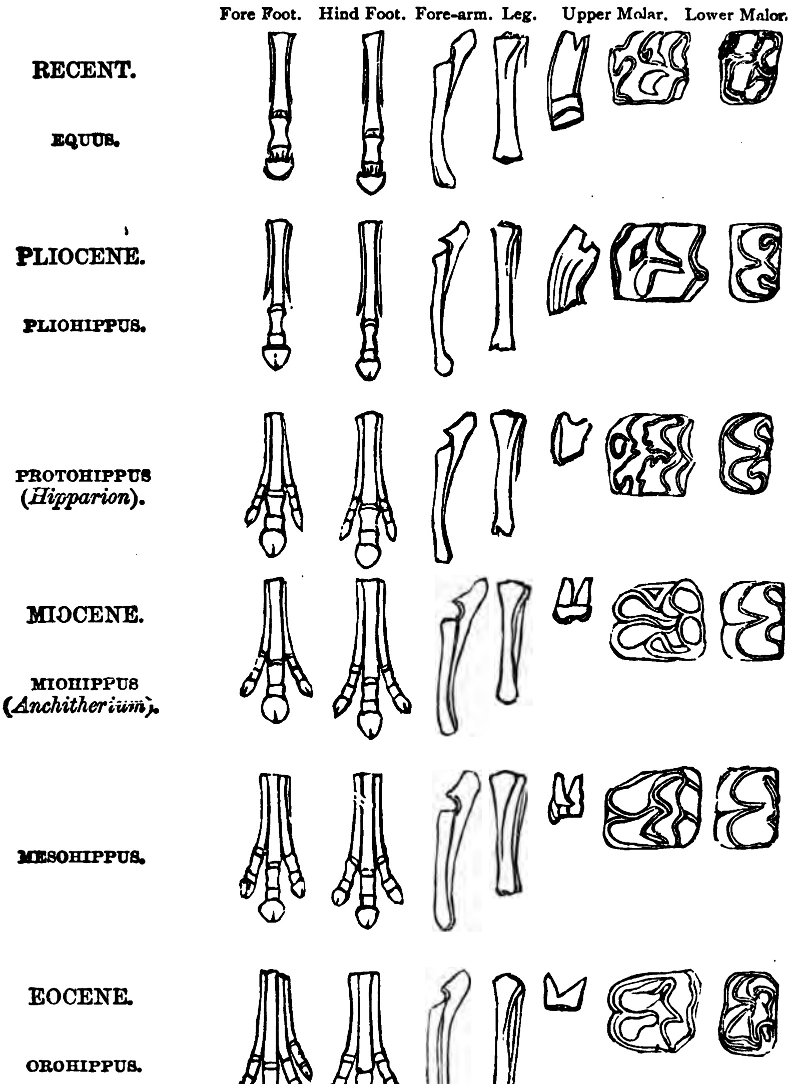


FIG 9.

Thus, thanks to these important researches, it has become evident that, so far as our present knowledge extends, the history of the horse-type is exactly and precisely that which could have been predicted from a knowledge of the principles of evolution. And the knowledge we now possess justifies us completely in the anticipation, that when the still lower Eocene deposits, and those which belong to the Cretaceous epoch, have yielded up their remains of ancestral equine animals, we shall find, first,

form with four complete toes and a rudiment of the innermost or first digit in front, with, probably, a rudiment of the fifth digit in the hind foot;* while, in still older forms, the series of the digits will be more and more complete, until we come to the five toed animals, in which, if the doctrine of evolution is well founded, the whole series must have taken its origin.

That is what I mean by demonstrative evidence of evolution. An inductive hypothesis is said to be demonstrated when the facts are shown to be in entire accordance with it. If that is not scientific proof, there are no merely inductive conclusions which can be said to be proved. And the doctrine of evolution, at the present time, rests upon exactly as secure a foundation as the Copernican theory of the motions of the heavenly bodies did at the time of its promulgation. Its logical basis is precisely of the same character—the coincidence of the observed facts with theoretical requirements.

The only way of escape, if it be a way of escape, from the conclusions which I have just indicated, is the supposition that all these different equine forms have been created separately at separate epochs of time; and, I repeat, that of such an hypothesis as this there neither is, nor can be, any scientific evidence; and, assuredly, so far as I know, there is none which is supported, or pretends to be supported, by evidence or authority of any other kind. I can but think that the time will come when such suggestions as these, such obvious attempts to escape the force of demonstration, will be put upon the same footing as the supposition made by some writers, who are, I believe, not completely extinct at present, that fossils are mere simulacra, are no indications of the former existence of the animals to which they seem to belong; but that they are either sports of Nature, or special creations, intended—as I heard suggested the other day—to test our faith.

In fact, the whole evidence is in favor of evolution, and there is none against it. And I say this, although perfectly well aware of the seeming difficulties which have been built up upon what appears to the uninformed to be a solid foundation. I meet constantly with the argument that the doctrine of evolution cannot be well founded, because it requires the lapse of a very vast period of time; while the duration of life upon the earth, thus implied, is inconsistent with the conclusions arrived at by the astronomer and the physicist. I may venture to say that I am familiar with those conclusions, inasmuch as some years ago, when President of the Geological Society of London, I took the liberty of criticising them, and of showing in what respects, as it appeared to me, they lacked complete and thorough demonstration. But, putting that point aside, suppose that, as the astronomers, or some of them, and some physical philosophers, tell us it is impossible that life could have endured upon the earth for as long a period as is required by the doctrine of evolution—supposing that to be proved—I desire to be informed what is the foundation for the statement that evolution does require so great a time? The biologist knows nothing whatever of the amount of time which may be required for the process of evolution. It is a matter of fact that the equine forms, which I have described to you, occur in the order stated in the Tertiary formations. But I have not the slightest means of guessing whether it took a million of years, or ten millions, or a hundred millions, or a thousand millions of years, to give rise to that series of changes. A biologist has no means of arriving at any conclusion as to the amount of time which may be needed for a certain

*Since this lecture was delivered, Professor Marsh has discovered a new genus of equine mammals (*Eohippus*) from the lowest Eocene deposits of the West, which corresponds very nearly to this description.—*American Journal of Science*, November, 1876.

quantity of organic change. He takes this time from the geologist. The geologist, considering the rate at which deposits are formed and the rate at which denudation goes on upon the surface of the earth, arrives at more or less justifiable conclusions as to the time which is required for the deposit of a certain thickness of rocks; and if he tells me that the Tertiary formations required 500,000,000 years for their deposit, I suppose he has good ground for what he says, and I take that as a measure of the duration of the evolution of the horse from the *Orohippus* up to its present condition. And, if he is right, undoubtedly evolution is a very slow process, and requires a great deal of time. But suppose, now, that an astronomer or a physicist—for instance, my friend Sir William Thomson—tells me that my geological authority is quite wrong; and that he has weighty evidence to show that life could not possibly have existed upon the surface of the earth 500,000,000 years ago, because the earth would have then been too hot to allow of life, my reply is: "That is not my affair; settle that with the geologist, and when you have come to an agreement among yourselves I will adopt your conclusion." We take our time from the geologists and physicists; and it is monstrous that, having taken our time from the physical philosopher's clock, the physical philosopher should turn round upon us, and say we are too fast or too slow. What we desire to know is, is it a fact that evolution took place? As to the amount of time which evolution may have occupied, we are in the hands of the physicists and astronomers, whose business it is to deal with those questions.

I have now, ladies and gentlemen, arrived at the conclusion of the task which I set before myself when I undertook to deliver these lectures. My purpose has been, not to enable those among you who have paid no attention to these subjects before, to leave this room in a condition to decide upon the validity or the invalidity of the hypothesis of evolution; but I have desired to put before you the principles upon which all hypotheses respecting the history of Nature must be judged; and furthermore, to make apparent the nature of the evidence and the amount of cogency which is to be expected and maybe obtained from it. To this end, I have not hesitated to regard you as genuine students and persons desirous of knowing the truth. I have not shrunk from taking you through long discussions, that I fear may have sometimes tried your patience; and I have inflicted upon you details which were indispensible, but which may well have been wearisome. But I shall rejoice—I shall consider that I have done you the greatest service which it was in my power to do—if I have thus convinced you that the great question which we have been discussing is not one to be dealt with by rhetorical flourishes, or by loose and superficial talk; but that it requires the keen attention of the trained intellect and the patience of the accurate observer.

When I commenced this series of lectures, I did not think it necessary to preface them with a prologue, such as might be expected from a stranger and a foreigner; for during my brief stay in your country, I have found it very hard to believe that a stranger could be possessed of so many friends, and almost harder that a foreigner could express himself in your language in such a way as to be, to all appearances, so readily intelligible. So far as I can judge, that most intelligent, and, perhaps, I may add, most singularly active and enterprising body, your press reporters, do not seem to have been deterred by my accent from giving the fullest account of everything that I happen to have said.

But the vessel in which I take my departure is even now ready to slip her moorings; I awake from my delusion that I am other than a stranger and a foreigner. I am ready to go back to

my place and country; but, before doing so, let me, by way of epilogue, tender to you my most hearty thanks for the kind and cordial reception which you have accorded to me; and let me thank you still more for that which is the greatest compliment which can be afforded to any person in my position—the continuous and undisturbed attention which you have bestowed upon the long argument which I have had the honor to lay before you.

ON THE STUDY OF BIOLOGY.

A Lecture delivered on the occasion of an Exposition of Scientific Apparatus, in the South Kensington Museum, London, in 1876.

It is my duty to-night to speak about the study of Biology, and while it may be that there are many of my audience who are quite familiar with that study; yet as a lecturer of some standing, it would, I know by experience, be very bad policy on my part to suppose such to be extensively the case. On the contrary, I must imagine that there are many of you who would like to know what Biology is; that there are others who have that amount of information, but would nevertheless gladly hear why it should be worth their while to study Biology; and yet others, again, to whom these two points are clear, but who desire to learn how they had best study it, and, finally, when they had best study it.

I shall, therefore, address myself to the endeavor to give you some answer to these four questions—what Biology is; why it should be studied; how it should be studied; and when it should be studied.

In the first place, in respect to what Biology is, there are, I believe, some persons who imagine that the term "Biology" is simply a newfangled denomination, a neologism in short, for what used to be known under the title of "Natural History;" but I shall try to show you, on the contrary, that the word is the expression of the growth of science during the last 200 years, and came into existence half a century ago.

At the revival of learning, knowledge was divided into two kinds—the knowledge of nature and the knowledge of man; for it was the current idea then (and a great deal of that ancient conception still remains) that there was a sort of essential antithesis, not to say antagonism, between nature and man; and that the two had not very much to do with one another, except that the one was oftimes exceedingly troublesome to the other. Though it is one of the salient merits of our great philosophers of the seventeenth century, that they recognized but one scientific method, applicable alike to man and to nature, we find this notion of the existence of a broad distinction between nature and man in the writings both of Bacon and of Hobbes of Malmesbury; and I have brought with me that famous work which is now so little known, greatly as it deserves to be studied, "The Leviathan," in order that I may put to

you in the wonderfully terse and clear language of Thomas Hobbes, what was his view of the matter. He says:—

"The register of knowledge of fact is called history. Whereof there be two sorts, one called natural history; which is the history of such facts or effects of nature as have no dependence on man's will; such as are the histories of metals, plants, animals, regions, and the like. The other is civil history; which is the history of the voluntary actions of men in commonwealths."

So that all history of fact was divided into these two great groups of natural and of civil history. The Royal Society was in course of foundation about the time that Hobbes was writing this book, which was published in 1651; and that Society was termed a "Society for the Improvment of Natural Knowledge," which was then nearly the same thing as a "Society for the Improvement of Natural History." As time went on, and the various branches of human knowledge became more distinctly developed and separated from one another, it was found that some were much more susceptible of precise mathematical treatment than others. The publication of the "Principia" of Newton, which probably gave a greater stimulus to physical science than any work ever published before, or which is likely to be published hereafter, showed that precise mathematical methods were applicable to those branches of science such as astronomy, and what we now call physics, which occupy a very large portion of the domain of what the older writers understood by natural history. And inasmuch as the partly deductive and partly experimental methods of treatment to which Newton and others subjected these branches of human knowledge, showed that the phenomena of nature which belonged to them were susceptible of explanation, and thereby came within the reach of what was called "philosophy" in those days; so much of this kind of knowledge as was not included under astronomy came to be spoken of as "natural philosophy"—a term which Bacon had employed in a much wider sense. Time went on, and yet other branches of science developed themselves. Chemistry took a definite shape; and since all these sciences, such as astronomy, natural philosophy, and chemistry, were susceptible either of mathematical treatment or of experimental treatment, or of both, a broad distinction was drawn between the experimental branches of what had previously been called natural history and the observational branches—those in which experiment was (or appeared to be) of doubtful use, and where, at that time, mathematical methods were inapplicable. Under these circumstances the old name of "Natural History" stuck by the residuum, by those phenomena, which were not, at that time, susceptible of mathematical or experimental treatment; that is to say, those phenomena of nature which come now under the general heads of physical geography, geology, mineralogy, the history of plants, and the history of animals. It was in this sense that the term was understood by the great writers of the middle of the last century—Buffon and Linnæus—by Buffon in his great work, the "Histoire Naturelle Generale," and by Linnæus in his splendid achievement, the "Systema Naturæ." The subjects they deal with are spoken of as "Natural History," and they called themselves and were called "Naturalists." But you will observe that this was not the original meaning of these terms; but that they had, by this time, acquired a signification widely different from that which they possessed primitively.

The sense in which "Natural History" was used at the time I am now speaking of has, to a certain extent, endured to the present day. There are now in existence in some of our northern universities, chairs of "Civil and Natural History," in which "Natural History" is used to indicate ex-

actly what Hobbes and Bacon meant by that term. The uhappy incumbent of the chair of Natural History is, or was, supposed to cover the whole ground of geology, mineralogy, and zoology, perhaps even botany, in his lectures.

But as science made the marvellous progress which it did make at the latter end of the last and the beginning of the present century, thinking men began to discern that under this title of "Natural History" there were included very heterogeneous constituents—that, for example, geology and mineralogy were, in many respects, widely different from botany and zoology; that a man might obtain an extensive knowledge of the structure and funtions of plants and animals, without having need to enter upon the study of geology or mineralogy, and *vice versâ;* and, further, as knowledge advanced, it became clear that there was a great analogy, a very close alliance, between those two sciences of botany and zoology which deal with living beings, while they are much more widely separated from all other studies. It is due to Buffon to remark that he clearly recognized this great fact. He says: "Ces deux genres d'êtres organisés [les animaux et les végétaux] ont beaucoup plus de propriétés communes que de différences réeles." Therefore, it is not wonderful, that at the beginning of the present century, in two different countries, and so far as I know, without any intercommunication, two famous men clearly conceived the notion of uniting the sciences which deal with living matter into one whole, and of dealing with them as one dicipline. In fact, I may say there were three men to whom this idea occurred contemporaneously, although there were but two who carried it into effect, and only one who worked it out completely. The persons to whom I refer were the eminent physiologist Bichat, and the great naturalist Lamarck, in France; and a distinguished German, Treviranus. Bichat* assumed the existence of a special group of "physiological" sciences. Lamarck, in a work published in 1801,† for the first time made use of the name "Biologie," from the two Greek words which signify a discourse upon life and living things. About the same time it occurred to Treviranus, that all those sciences which deal with living matter are essentially and fundamentally one, and ought to be treated as a whole; and, in the year 1802, he published the first volume of what he also called "Biologie." Treviranus's great merit lies in this, that he worked out his idea and wrote the very remarkable book to which I refer. It consists of six volumes, and occupied its author for twenty years—from 1802 to 1822.

That is the origin of the term "Biology;" and that is how it has come about that all clear thinkers and lovers of consistent nomenclature have substituted for the old confusing name of "Natural History," which has conveyed so many meanings, the term "Biology," which denotes the whole of the sciences which deal with living things, whether they be animals or whether they be plants. Some little time ago—in the course of this year, I think—I was favored by a learned classic, Dr. Field of Norwich, with a disquisition, in which he endeavored to prove that, from a philological point of view, neither Treviranus nor Lamarck had any right to coin this new word "Biology" for their purpose; that, in fact, the Greek word "Bios" had relation only to human life and human affairs, and that a different word was employed by the Greeks when they wished to speak of the life of animals and plants. So Dr. Field tells us we are all wrong in using the term biology, and that we ought to employ another; only he is not quite sure

* See the distinction between the "sciences physiques" and the "sciences physiologiques" in the "Anatomie Generale," 1881.

† "Hydrogeologie," an. x (1801).

about the propriety of that which he proposes as a substitute. It is a somewhat hard one—"zootocology." I am sorry we are wrong, because we are likely to continue so. In these matters we must have some sort of "Statute of Limitations." When a name has been employed for half a-century, persons of authority * have been using it, and its sense has become well understood, I am afraid that people will go on using it, whatever the weight of philological objection.

Now that we have arrived at the origin of this word "Biology," the next point to consider is: What ground does it cover? I have said that, in its strict technical sense, it denotes all the phenomena which are exhibited by living things, as distinguished from those which are not living; but while that is all very well, so long as we confine ourselves to the lower animals and to plants, it lands us in considerable difficulties when we reach the higher forms of living things. For whatever view we may entertain about the nature of man, one thing is perfectly certain, that he is a living creature. Hence, if our definition is to be interpreted strictly, we must include man and all his ways and works under the head of Biology; in which case, we should find that psychology, politics, and political economy would be absorbed into the province of Biology. In fact, civil history would be merged in natural history. In strict logic it may be hard to object to this course, because no one can doubt that the rudiments and outlines of our own mental phenomena are traceable among the lower animals. They have their economy and their polity, and if, as is always admitted, the polity of bees and the commonwealth of wolves fall within the purview of the biologist proper, it becomes hard to say why we should not include therein human affairs, which in so many cases resemble those of the bees in zealous getting, and are not without a certain parity in the proceedings of the wolves. The real fact is that we biologists are a self-sacrificing people; and inasmuch as, on a moderate estimate, there are about a quarter of a million different species of animals and plants to know about already, we feel that we have more than sufficient territory. There has been a sort of practical convention by which we give up to a different branch of science what Bacon and Hobbs would have called "Civil History." That branch of science has constituted itself under the head of Sociology. I may use phraseology which, at present, will be well understood, and say that we have allowed that province of Biology to become autonomous; but I should like you to recollect that that is a sacrifice, and that you should not be surprized if it occasionally happens that you see a biologist apparently trespassing in the region of philosophy or politics; or meddling with human education; because, after all, that is a part of his kingdom which he has only voluntarily forsaken.

Having now defined the meaning of the word Biology, and having indicated the general scope of Biological Science, I turn to my second question, which is—Why should we study Biology? Possibly the time may come when that will seem a very odd question. That we, living creatures, should not feel a certain amount of interest in what it is that constitutes our life will eventually, under altered ideas of the fittest objects of human inquiry, appear to be a singular phenomenon; but at present, judging by the practice of teachers and educators, Biology would seem to be a topic that does not concern us at all. I propose to put before you a few considerations with which I dare say many will be familiar already, but which will suffice to show—not fully, because to demonstrate this point fully would take a great many lectures—that there are

* "The term *Biology*, which means exactly what we wish to express, *the Science of Life*, has often been used, and has of late become not uncommon among good writers."—Whewell, "Philosophy of the Inductive Sciences," vol. i. p. 544 (edition of 1847).

some very good and substantial reasons why it may be advisable that we should know something about this branch of human learning.

I myself entirely agree with another sentiment of the philosopher of Malmesbury, "that the scope of all speculation is the performance of some action or thing to be done," and I have not any very great respect for, or interest in, mere knowing as such. I judge of the value of human pursuits by their bearing upon human interests; in other words, by their utility; but I should like that we should quite clearly understand what it is that we mean by this word "utility." In an Englishman's mouth it generally means that by which we get pudding or praise, or both. I have no doubt that is one meaning of the word utility, but it by no means includes all I mean by utility. I think that knowledge of every kind is useful in proportion as it tends to give people right ideas, which are essential to the foundation of right practice, and to remove wrong ideas, which are the no less essential foundations and fertile mothers of every description of error in practice. And inasmuch as, whatever practical people may say, this world is, after all, absolutely governed by ideas, and very often by the wildest and most hypothetical ideas, it is a matter of the very greatest importance that our theories of things, and even of things that seem a long way apart from our daily lives, should be as far as possible true, and as far as possible removed from error. It is not only in the coarser practical sense of the word "utility," but in this higher and broader sense, that I measure the value of the study of biology by its utility; and I shall try to point out to you that you will feel the need of some knowledge of biology at a great many turns of this present nineteenth century life of ours. For example, most of us attach great importance to the conception which we entertain of the position of man in this universe, and his relation to the rest of nature. We have almost all been told, and most of us hold by the tradition, that man occupies an isolated and peculiar position in nature; that though he is in the world, he is not of the world; that his relations to things about him are of a remote character; that his origin is recent, his duration likely to be short, and that he is the great central figure round which other things in this world revolve. But this is not what the biologist tells us.

At the present moment you will be kind enough to separate me from them, because it is in no way essential to my present argument that I should advocate their views. Don't suppose that I am saying this for the purpose of escaping the responsibility of their beliefs; indeed, at other times, and in other places, I do not think that point has been left doubtful; but I want clearly to point out to you that for my present argument they may all be wrong; and, nevertheless, my argument will hold good. The biologists tell us that all this is an entire mistake. They turn to the physical organization of man. They examine his whole structure, his bony frame and all that clothes it. They resolve him into the finest particles into which the microscope will enable them to break it up. They consider the performance of his various functions and activities, and they look at the manner in which he occurs on the surface of the world. Then they turn to other animals, and taking the first handy domestic animal—say a dog—they profess to be able to demonstrate that the analysis of the dog leads them, in gross, to precisely the same results as the analysis of the man; that they find almost identically the same bones, having the same relations; that they can name the muscles of the dog by the names of the muscles of the man, and the nerves of the dog by those of the nerves of the man, and that such structures and organs of the sense as we find in the man such also we find in the dog; they analyze the brain and spinal cord, and they find that the momenclature which fits the one answers for the other. They

carry their microscopic inquiries in the case of the dog as far as they can, and they find that his body is resolvable into the same elements as those of the man. Moreover, they trace back the dog's and the man's development, and they find that, at a certain stage of their existence, the two creatures are not distinguishable the one from the other; they find that the dog and his kind have a certain distribution over the surface of the world, comparable in its way to the distribution of the human species. What is true of the dog they tell us is true of all the higher animals; and they assert that they can lay down a common plan for the whole of these creatures, and regard the man and the dog, the horse and the ox as minor modifications of one great fundamental unity. Moreover, the investigations of the last three-quarters of a century have proved, they tell us, that similar inquiries, carried out through all the different kinds of animals which are met with in nature, will lead us, not in one straight series, but by many roads, step by step, gradation by gradation, from man, at the summit, to specks of animated jelly at the bottom of the series. So that the idea of Leibnitz, and of Bonnet, that animals form a great scale of being, in which there are a series of gradations from the most complicated form to the lowest and simpest; that idea, though not exactly in the form in which it was propounded by those philosophers, turns out to be substantially correct. More than this, when biologists pursue their investigations into the vegetable world, they find that they can, in the same way, follow out the structure of the plant, from the most gigantic and complicated trees down through a similar series of gradations, until they arrive at specks of animated jelly, which they are puzzled to distinguish from those specks which they reached by the animal road.

Thus, biologists have arrived at the conclusion that a fundamental uniformity of structure pervades the animal and vegetable worlds, and that plants and animals differ from one another simply as diverse modifications of the same great general plan.

Again, they tell us the same story in regard to the study of function. They admit the large and inportant interval which, at the present time, separates the manifestations of the mental faculties, observable in the higher forms of mankind, and even in the lower forms, such as we know them, from those exhibited by other animals; but, at the same time, they tell us that the foundations, or rudiments, of almost all the faculties of man are to be met with in the lower animals; that there is a unity of mental faculty as well as of bodily structure, and that, here also, the difference is a difference of degree and not of kind. I said "almost all," for a reason Among the many distinctions which have been drawn between the lower creatures and ourselves, there is one which is hardly ever insisted on,* but which may be very fitly spoken of in a place so largely devoted to Art as that in which we are assembled. It is this, that while, among various kinds of animals, it is possible to discover traces of all the other faculties of man, especially the faculty of mimicry, yet that particular form of mimicry which shows itself in the imitation of form, either by modelling or by drawing, is not to be met with. As far as I know, there is no sculpture or modelling, and decidedly no painting or drawing, of animal origin. I mention the fact, in order that such comfort may be derived therefrom as artists may feel inclined to take.

If what the biologists tell us is true, it will be needful to get rid of our erroneous conceptions of man, and of his place in nature, and to substitute right ones for them. But it is impossible to form any judgment as to whether the biologists are right or

* I think that my friend Professor Allman was the first to draw attention to it.

wrong, unless we are able to appreciate the nature of the arguments which they have to offer.

One would almost think this to be a self-evident proposition. I wonder what a scholar would say to the man who should undertake to criticise a difficult passage in a Greek play, but who obviously had not acquainted himself with the rudiments of Greek grammar. And yet, before giving positive opinions about these high questions of Biology, people not only do not seem to think it necessary to be acquainted with the grammar of the subject, but they have not even mastered the alphabet. You find criticism and denunciation showered about by persons, who not only have not attempted to go through the discipline necessary to enable them to be judges, but who have not even reached that stage of emergence from ignorance in which the knowledge that such a discipline is necessary dawns upon the mind. I have had to watch with some attention—in fact I have been favored with a good deal of it myself—the sort of criticism with which biologists and biological teachings are visited. I am told every now and then that there is a "brilliant article"* in so-and-so, in which we are all demolished. I used to read these things once, but I am getting old now, and I have ceased to attend very much to this cry of "wolf." When one does read read any of these productions, what one finds generally, on the face of it, is that the brilliant critic is devoid of even the elements of biological knowledge, and that his brilliancy is like the light given out by the crackling of thorns under a pot of which Solomon speaks. So far as I recollect, Solomon makes use of the image for the purposes of comparison; but I will not proceed further into that matter.

Two things must be obvious: in the first place, that every man who has the interests of truth at heart must earnestly desire that every well-founded and just criticism that can be made should be made; but that, in the second place, it is essential to anybody's being able to benefit by criticism, that the critic should know what he is talking about, and be in a position to form a mental image of the facts symbolized by the words he uses. If not, it is obvious in the case of a biological argument, as it is in that of a historical or philological discussion, that such criticism is a mere waste of time on the part of its author, and wholly undeserving of attention on the part of those who are criticised! Take it then as an illustration of the importance of biological study, that thereby alone are men able to form something like a rational conception of what constitutes valuable criticism of the teachings of biologists.*

* Galileo was troubled by a sort of people whom he called "paper philosophers," because they fancied that the true reading of nature was to be detected by the collation of texts. The race is not extinct, but, as of old, brings forth its "winds of doctrine" by which the weathercock heads among us are much exercised.

*Some critics do not even take the trouble to read. I have recently been adjured with much solemnity, to state publicly why I have "changed my opinion" as to the value of the palæontological evidence of the occurrence of evolution.

To this my reply is, Why should I, when that statement was made seven years ago? An address delivered from the Presidential Chair of the Geological Society, in 1870, may be said to be a public document, inasmuch as it not only appeared in the *Journal* of that learned body, but was re-published, in 1873, in a volume of "Critiques and Addresses," to which my name is attached. Therein will be found a pretty full statement of my reasons for enunciating two propositions: (1) that "when we turn to the higher *Vertebrata*, the results of recent investigations, however we may sift and criticise them, seem to me to leave a clear balance in favor of the evolution of living forms one from another;" and (2) that the case of the horse is one which "will stand rigorous criticism."

Thus I do not see clearly in what way I can be said to have changed my opinion, except in the way of intensifying it,

Next, I may mention another bearing of biological knowledge—a more practical one in the ordinary sense of the word. Consider the theory of infectious disease. Surely that is of interest to all of us. Now, the theory of infectious disease is rapidly being elucidated by biological study. It is possible to produce, from among the lower animals, examples of devastating diseases which spread in the same manner as our infectious disorders, and which are certainly and unmistakably caused by living organisms. This fact renders it possible, at any rate, that that doctrine of the causation of infectious disease which is known under the name of "the germ theory" may be well-founded; and, if so, it must needs lead to the most important practical measures in dealing with those terrible visitations. It may be well that the general, as well as the professional, public should have a sufficient knowledge of biological truths to be able to take a rational interest in the discussion of such problems, and to see, what I think they may hope to see, that, to those who possess a sufficient elementary knowledge of Biology, they are not all quite open questions.

Let me mention another important practical illustration of the value of biological study. Within the last forty years the theory of agriculture has been revolutionized. The researches of Liebig, and those of our own Lawes and Gilbert, have had a bearing upon that branch of industry the importance of which cannot be overestimated; but the whole of these new views have grown out of the better explanation of certain processes which go on in plants; and which, of course, form a part of the subject matter of Biology.

I might go on multiplying these examples, but I see that the clock won't wait for me, and I must therefore pass to the third question to which I referred: Granted that Biology is something worth studying, what is the best way of studying it? Here I must point out that, since Biology is a physical science, the method of studying it must needs be analogous to that which is followed in the other physical sciences. It has now long been recognized that, if a man wishes to be a chemist, it is not only necessary that he should read chemical books and attend chemical lectures, but that he should actually perform the fundamental experiments in the laboratory for himself, and thus learn exactly what the words which he finds in his books and hears from his teachers mean. If he does not do so, he may read till the crack of doom, but he will never know much about chemistry. That is what every chemist will tell you, and the physicist will do the same for his branch of science. The great changes and improvements in physical and chemical scientific education, which have taken place of late, have all resulted from the combination of practical teaching with the reading of books and with the hearing of lectures. The same thing is true in Biology. Nobody will ever know anything about Biology except in a dilettante "paper-philosopher" way, who contents himself with reading books on botany, zoology, and the like; and the reason of this is simple and easy to understand. It is that all language is merely symbolical of the things of which it treats; the more complicated the things, the more bare is the symbol, and the more its verbal definition requires to be supplemented by the information derived directly from the handling, and the seeing, and the touching of the thing symbolized: that is really what is at the bottom of the whole matter. It is plain common sense, as all truth, in the long run, is only common sense clarified. If you want a man to be a tea merchant, you don't tell him to read books about China or about tea, but you put him into a tea mer-

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when in consequence of the accumulation of similar evidence since 1870, I recently spoke of the denial of evolution as not worth serious consideration.

chant's office, where he has the handling, the smelling, and the tasting of tea. Without the sort of knowledge which can be gained only in this practical way, his exploits as a tea merchant will soon come to a bankrupt termination. The "paper-philosophers" are under the delusion that physical science can be mastered as literary accomplishments are acquired, but unfortunately it is not so. You may read any quantity of books, and you may be almost as ignorant as you were at starting, if you don't have, at the back of your minds, the change for words in definite images which can only be acquired through the operation of your observing faculties on the phenomena of nature.

It may be said:—"That is all very well, but you told us just now that there are probably something like a quarter of a million different kinds of living and extinct animals and plants, and a human life could not suffice for the examination of one-fiftieth part of all these." That is true, but then comes the great inconvenience of the way things are arranged; which is, that although there are these immense numbers of different kinds of living thing in existence, yet they are built up, after all, upon marvellously few plans.

There are certainly more than 100,000 species of insects, and yet anybody who knows one insect—if a properly chosen one—will be able to have a very fair conception of the structure of the whole. I do not mean to say to say that he will know that structure thoroughly, or as well as it is desirable he should know it; but he will have enough real knowledge to enable him to understand what he reads, to have genuine images in his mind of those structures which become so variously modified in all the forms of insects he has not seen. In fact there are such things as types of form among animals and vegetables, and for the purpose of getting a definite knowledge of what constitutes the leading modifications of animal and plant life, it is not needful to examine more than a comparatively small number of animals and plants.

Let me tell you what we do in the biological laboratory which is lodged in a building adjacent to this. There I lecture to a class of students daily for about four-and-a half months and my class have, of course, their text-books; but the essential part of the whole teaching, and that which I regard as really the most important part of it, is a laboratory for practical work, which is simply a room with all the appliances needed for ordinary dissection. We have tables properly arranged in regard to light, microscopes, and dissecting instruments, and we work through the structure of a certain number of animals and plants. As, for example, among the plants, we take a yeast plant, a *Protococcus*, a common mould, a *Chara*, a fern, and some flowering plant; among animals we examine such things as an *Amœba*, a *Vorticella*, and a fresh-water polype. We dissect a star-fish, an earthworm, a snail, a squid, and a fresh-water mussel. We examine a lobster and a cray-fish, and a black-beetle. We go on to a common skate, a codfish, a frog, a tortoise, a pigeon, and a rabbit, and that takes us about all the time we have to give. The purpose of this course is not to make skilled dissectors, but to give every student a clear and definite conception, by means of sense-images, of the characteristic structure of each of the leading modifications of the animal kingdom; and that is perfectly possible, by going no further than the length of that list of forms which I have enumerated. If a man knows the structure of the animals I have mentioned, he has a clear and exact, however limited, apprehension of the essential features of the organization of all those great divisions of the animal and vegetable kingdoms to which the forms I have mentioned severally belong. And it then becomes possible for him to read with profit; because every time he meets with the name of a structure,

he has a definite image in his mind of what the name means in the particular creature he is reading about, and therefore the reading is not mere reading. It is not mere repetition of words; but every term employed in the description, we will say, of a horse, or of an elephant, will call up the image of the things he had seen in the rabbit, and he is able to form a distinct conception of that which he has not seen, as a modification of that which he has seen.

I find this system to yield excellent results; and I have no hesitation whatever in saying that any one who has gone through such a course, attentively, is in a better position to form a conception of the great truths of Biology, especially of morphology (which is what we chiefly deal with), than if we merely read all the books on that topic put together.

The connection of this discourse with the Loan Collection of Scientific Apparatus arises out of the exhibition in that collection of certain aids to our laboratory work. Such of you as have visited that very interesting collection may have noticed a series of diagrams and of preparations illustrating the structure of a frog. Those diagrams and preparations have been made for the use of the students in the biological laboratory. Similar diagrams and preparations illustrating the structure of all the other forms of life we examine, are either made or in course of preparation. Thus the student has before him, first, a picture of the structure he ought to see; secondly, the structure itself worked out; and if with these aids, and such needful explanations and practical hints as a demonstrator can supply, he cannot make out the facts for himself in the materials supplied to him, he had better take to some other pursuit than that of biological science.

I should have been glad to have said a few words about the use of museums in the study of Biology, but I see that my time is becoming short, and I have yet another question to answer. Nevertheless I must, at the risk of wearying you, say a word or two upon the important subject of museums. Without doubt there are no helps to the study of Biology, or rather to some branches of it, which are, or may be, more important than natural history museums; but, in order to take this place in regard to Biology, they must be museums of the future. The museums of the present do not, by any means, do so much for us as they might do. I do not wish to particularize, but I dare say many of you, seeking knowledge, or in the laudable desire to employ a holiday usefully, have visited some great natural history museum. You have walked through a quarter of a mile of animals, more or less well stuffed, with their long names written out underneath them; and, unless your experience is very different from that of most people, the upshot of it all is that you leave that splendid pile with sore feet, a bad headache, and a general idea that the animal kingdom is a "mighty maze without a plan." I do not think that a museum which brings about this result does all that may be reasonably expected from such an institution. What is needed in a collection of natural history is that it should be made as accessible and as useful as possible, on the one hand to the general public, and on the other to scientific workers. That need is not met by constructing a sort of happy hunting-ground of miles of glass cases; and, under the pretence of exhibiting everything, putting the maximum amount of obstacle in the way of those who wish properly to see anything.

What the public want is easy and unhindered access to such a collection as they can understand and appreciate; and what the men of science want is similar access to the materials of science. To this end the vast mass of objects of natural history should be divided into two parts—one open to the public, the other to men of science, every day. The former division should exemplify

all the more important and interesting forms of life. Explanatory tablets should be attached to them, and catalogues containing clearly-written, popular expositions of the general significance of the objects exhibited should be provided. The latter should contain, packed into a comparatively small space, in rooms adapted for working purposes, the objects of purely scientific interest. For example, we will say I am an ornithologist. I go to examine a collection of birds. It is a positive nuisance to have them stuffed. It is not only sheer waste, but I have to reckon with the ideas of the bird-stuffer, while, if I have the skin and nobody has interfered with it, I can form my own judgment as to what the bird was like. For ornithological purposes, what is needed is not glass cases full of stuffed birds on perches, but convenient drawers into each of which a great quantity of skins will go. They occupy no great space, and do not require any expenditure beyond their original cost. But for the edification of the public, who want to learn indeed, but do not seek for minute and technical knowledge, the case is different. What one of the general public walking into a collection of birds desires to see is not all the birds that can be got together. He does not want to compare a hundred species of the sparrow tribe side by side; but he wishes to know what a bird is, and what are the great modifications of bird structure, and to be able to get at that knowledge easily. What will best serve his purpose is a comparatively small number of birds carefully selected, and artistically, as well as accurately, set up; with their different ages, their nests, their young, their eggs, and their skeletons side by side; and in accordance with the admirable plan which is pursued in this museum, a tablet, telling the spectator in legible characters what they are and what they mean. For the instruction and recreation of the public such a typical collection would be of far greater value than any many-acred imitation of Noah's ark.

Lastly comes the question as to when biological study may best be pursued. I do not see any valid reason why it should not be made, to a certain extent, a part of ordinary school training. I have long advocated this view, and I am perfectly certain that it can be carried out with ease, and not only with ease, but with very considerable profit to those who are taught; but then such instruction must be adapted to the minds and needs of the scholars. They used to have a very odd way of teaching the classical languages when I was a boy. The first task set you was to learn the rules of the Latin grammar in the Latin language—that being the language you were going to learn! I thought then that this was an odd way of learning a language, but did not venture to rebel against the judgment of my superiors. Now, perhaps, I am not so modest as I was then, and I allow myself to think that it was a very absurd fashion. But it would be no less absurd, if we were to set about teaching Biology by putting into the hands of boys a series of definitions of the classes and orders of the animal kingdom, and making them repeat them by heart. That is so very favorite a method of teaching, that I sometimes fancy the spirit of the old classical system has entered into the new scientific system, in which case I would much rather that any pretence at scientific teaching were abolished altogether. What really has to be done is to get into the young mind some notion of what animal and vegetable life is. In this matter, you have to consider practical convenience as well as other things. There are difficulties in the way of a lot of boys making messes with slugs and snails; it might not work in practice. But there is a very convenient and handy animal which everybody has at hand, and that is himself; and it is a very easy and simple matter to obtain common plants. Hence the general truths of anatomy and physi-

ology can be taught to young people in a very real fashion by dealing with the broad facts of human structure. Such viscera as they cannot very well examine in themselves, such as hearts, lungs, and livers, may be obtained from the nearest butcher's shop. In respect to teaching something about the biology of plants, there is no practical difficulty, because almost any of the common plants will do, and plants do not make a mess—at least they do not make an unpleasant mess; so that, in my judgment, the best form of Biology for teaching to very young people is elementary human physiology on the one hand, and the elements of botany on the other; beyond that I do not think it will be feasible to advance for some time to come. But then I see no reason why, in secondary schools, and in the Science Classes which are under the control of the Science and Art Department—and which I may say, in passing, have, in my judgment, done so very much for the diffusion of a knowledge of science over the country—we should not hope to see instruction in the elements of Biology carried out, not perhaps to the same extent, but still upon somewhat the same principle as here. There is no difficulty, when you have to deal with students of the ages of 15 or 16, in practicing a little dissection and in getting a notion of, at any rate, the four or five great modifications of the animal form; and the like is true in regard to the higher anatomy of plants.

While, lastly, to all those who are studying biological science with a view to their own edification merely, or with the intention of becoming zoologists or botanists; to all those who intend to pursue physiology—and especially to those who propose to employ the working years of their lives in the practice of medicine—I say that there is no training so fitted, or which may be of such important service to them, as the discipline in practical biological work which I have sketched out as being pursued in the laboratory hard by.

I may add that, beyond all these different classes of persons who may profit by the study of Biology, there is yet one other. I remember, a number of years ago, that a gentleman who was a vehement opponent of Mr. Darwin's views and had written some terrible articles against them, applied to me to know what was the best way in which he could acquaint himself with the strongest arguments in favor of evolution. I wrote back, in all good faith and simplicity, recommending him to go through a course of comparative anatomy and physiology, and then to study development. I am sorry to say he was very much displeased, as people often are with good advice. Notwithstanding this discouraging result, I venture, as a parting word, to repeat the suggestion, and to say to all the more or less acute lay and clerical "paper-philosophers"* who venture into the regions of biological controversy—Get a little sound, thorough, practical, elementary instruction in biology.

* Writers of this stamp are fond of talking about the Baconian method. I beg them, therefore, to lay to heart these two weighty sayings of the herald of Modern Science:

"Syllogismus ex propositionibus constat, propositiones ex verbis, verba notionum tesseræ sunt. Itaque si notiones ipsæ (*id quod basis rei est*) confusæ sint et temere a rebus abstractæ, nihil in iis quæ superstruuntur est firmitudinis.—"Novum Organon," ii. 14.

"Huic autem vanitati nonnulli ex modernis summa levitate ita indulserunt, ut in primo capitulo Geneseos et in libro Job et aliis scripturis sacris, philosophiam naturalem fundare conati sint; *inter vivos quærentes mortua.*"—*Ibid.*, 65.

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